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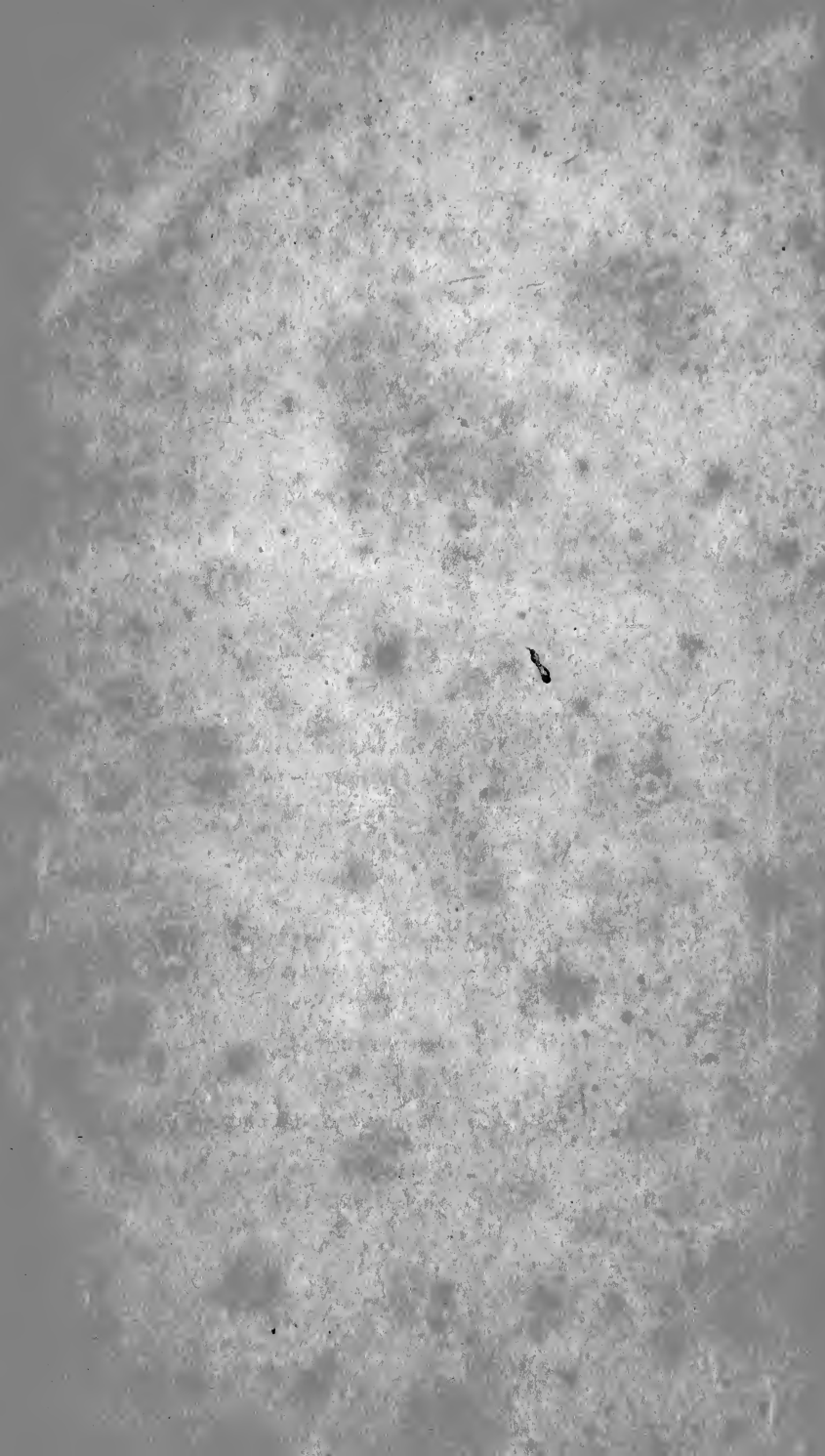
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REPLY

TO

DR. JAMES CARMICHAEL SMYTH,

CONTAINING

REMARKS

ON HIS LETTER TO MR. WILBERFORCE,

AND

A FURTHER ACCOUNT OF THE DISCOVERY OF THE POWER

OF

MINERAL ACIDS IN A STATE OF GAS

TO

DESTROY CONTAGION.

BY JOHN JOHNSTONE, M. D.

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, &c. &c.

Πονεῖ μὲν ἡ μέλισσα κηρίον μέλι
Φιλοφρονῶσα προσφύη συνεργάτιν,
Ἐπειπερ ἄλλω τῷ τευγῶντι χηρία,
Τὸ κέντρον ἐξέπεμψε, καὶ πλήττει μέγα.

Sennarii prefixed to Soranus and Oribasius, from the collection of Nicetas,
published at Florence 1754.

London :

PRINTED FOR J. MAWMAN, NO. 22, POULTRY.

1805.

By T. Gillet, Salisbury-Square.



RECEIVED

TO

THE UNITED STATES DEPARTMENT OF THE INTERIOR

WASHINGTON, D. C.

RECEIVED

ON THE 15TH DAY OF SEPTEMBER 1906

FOR THE RECORDS OF THE DEPARTMENT OF THE INTERIOR

TO

THE UNITED STATES DEPARTMENT OF THE INTERIOR

TO

1576

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PREFACE.

THE nature of the discussion contained in the following Pages will account for, and perhaps excuse, the length of this Publication. I have not shunned any one of the topics which Dr. Smyth has introduced into his Letter, nor have I entered upon any fresh matter unnecessarily. The manner of Dr. Smyth's attack required, that the subject should be worked agonistically.

That what I have written will produce conviction upon the minds of all men, I do scarcely flatter myself; for what proofs or what arguments can convince him who is interested in not believing, or determined not to believe? That it will establish my father's claim to discovery in the minds of all dispassionate and scientific enquirers I have no doubt.

I am

I am surpris'd at the slight mention of the power of mineral acid gases which has been lately made in certain publications, professedly written for the purpose of preventing the communication or spreading of pestilential contagion. Do the mineral acid gases destroy the matter of contagion or no? Surely if they do, they are agents capable of the most important medical application, and not to be overlooked or only slightly alluded to, in the prevention of contagious fevers. If they do not destroy it, the error ought instantly to be cleared up.

Nor do the strictest attention to cleanliness, and the most constant ventilation, altogether supersede the mineral acid gases as antiloimics no more than these supersede attention to cleanliness and ventilation. Cleanliness and pure air are necessary for the continuance of health, but they do not prevent the formation of contagious matter. They are only the
next

next aids after mineral acid gases for preventing its accumulation. By passing a current of air continually through the sick room, the pestilential air may be swept away. By removing the bed-clothes and the linen of the patient, the matter of contagion accumulated in them, may be removed too. But this cannot be done every moment; yet the matter of contagion is formed every moment. Here, then, is the essential agency of mineral acid gases. They do meet the matter of contagion every moment, and destroy it, as soon as it is formed.

I must, however, reserve the discussion of these collateral topics for another occasion. In importance, they yield to no subjects of merely human enquiry, and demand therefore to be discussed with candour and deliberation, without prejudice and without passion.

J. J.

Birmingham, Sept. 24, 1805.

ERRATA.

P. 46, line 5, for "quote" read "use."

P. 46, line 14, *dele* "father's."

P. 52, note, for "τρωνου" read "τρωναι."

P. 204, note, after "present" *dele* :-

REPLY

TO

DR. JAMES CARMICHAEL SMYTH'S

LETTER

TO

MR. WILBERFORCE.

IT is not denied that the first public mention of the respirable mineral acid gases, as correctors of putrefaction, was made by Dr. James Johnstone, sen. late physician in Worcester, in 1758. The second mention of them was made by M. Guyton de Morveau in 1773. In 1779 Dr. James Johnstone, jun. recommended muriatic acid gas, as the surest method to prevent the spread of contagion. In 1795 Dr.

B

James

James Carmichael Smyth recommended nitric acid gas, to destroy contagion. In 1802 the same Dr. James Carmichael Smyth petitioned the House of Commons to remunerate him, as the discoverer of the power of mineral acids in their gaseous state to destroy contagion. In 1803 I published my account of the discovery, and proved, I hope satisfactorily, the claim of my father to priority of discovery. In 1805 * Dr. Smyth, in answer to my pamphlet, published his letter to Mr. Wilberforce.

Had I only to reply to the *arguments* adduced by Dr. Smyth in this letter, against the facts, that prove my father to have discovered the power of mineral acids in a state of gas to destroy contagion, there would be no occasion

* These are the dates of the different publications ; but it may be proper for me here to intimate, that in some of the following pages I shall have occasion to adduce my reasons for believing, that Dr. Smyth did not employ *the respirable mineral acid gasses*, to destroy contagion at Winchester in 1780.

for long and elaborate discussion. By stating my case again—by correcting Dr. Smyth's misconception of that statement—by availing myself of the various documents which, after lapse of more than thirty years, I have been able to collect; during which period it was never foreseen that any dispute would arise on the subject of discovery of the principle, and consequently during which no precautions were taken against specious plagiarism, or ostentatious imposture--by bringing forward these documents as they were left by my father and my brother, undecorated, unvarnished, and written without any view to competition for reward, or controversy upon fact; the soundness of my father's claims, and the justness of my reasoning in defence of them, would easily be established.

But a more unpleasant and a more serious task is imposed upon me, by the *manner* in which Dr. Smyth has treated the subject, in

his letter to Mr. Wilberforce. Not content with the display of the whole force of his own peculiar logic, he attacks me also with the artillery of his rhetoric. The establishment of the plain matter of fact, whether my father did use the muriatic acid gas to correct or destroy contagion fifty years ago, is nearly overlooked by this renowned champion of discovery; who, resolved to gain the glorious meed, buckles on his armour and girds himself for battle, shouting defiance. Be it so:

..... σέθεν δ' ἐγὼ ἐκ ἀλεγίζω,

Οὐδ' ὀθομαι κλέοντος·

I shall not swerve from the tenor of my former statement,

Ἡ γάρ κεν δειλός τε καὶ ἐτιδανὸς καλεοίμην

Εἰ δὲ σοι πᾶν ἔργον ὑπείξομαι ὅττι κεν εἴπωις.

I am ready to meet *him* on fair and honourable grounds. But I cannot descend to miserable and unprofitable revilings; to cavilling about one solitary word in a quotation, when that word

is

is of little or no importance; to invectives, as giving aid to quibbles, and dignity to trifles; or to other petty and disingenuous expedients, which are too often permitted to disgrace the controversies of scholars, and men of science. It is, however, incumbent upon me to refute, as it is impossible for me not to resent, the invidious charge of being a wilful misquoter, to vindicate myself from the indirect imputation of falsehood, and to repel the direct and reiterated reproaches which Dr. Smyth has ventured to throw out upon my inaccuracy and my ignorance. Under such unexpected and unmerited provocation, I may sometimes feel myself warranted in accusing Dr. Smyth, as well as in defending myself. When I asserted the claim of my father, I did not admit one harsh expression into my statement, nor employ one unhandsome insinuation, that could in the slightest degree wound the delicacy of Dr. Smyth.

The

The question of discovery of the power of mineral acid gasses to destroy contagion, was first brought before the public by Dr. J. C. Smyth himself; and the manner and the terms of his petition evidently imply a discovery of the principle, not merely a variation of the agent. The report grounded upon that petition, substantially and expressly asserts the superiority of the agent, employed by Dr. Smyth, to the agent employed by my father; I therefore turned my attention to the subjects which the petition itself, and the report grounded upon it, presented to my mind. Looking to the petition, I maintained that the principle had been previously discovered by my father; looking to the report, I maintained, that the agent known to my father, and used by him, before either of the agents appears to have been known and used by Dr. Smyth, was of equal efficacy with that for which Dr. Smyth contended, and upon the discovery of which

he

he grounded his pretensions to parliamentary reward. But in no one passage did I then deny, that Dr. Smyth himself had by the strength of his own mind, or in the course of his own observations, struck out a similar discovery. I did not then dispute his claim to priority in the use of the nitric acid gas. I contended solely and unequivocally for my father's prior use of the muriatic. I did not represent Dr. Smyth as intentionally depreciating, or encouraging others to depreciate, the merit of my father; I did not scoff at Dr. Smyth's professional talents; I did not controvert the reality, or the magnitude of his professional services; I am not conscious of having spoken in any one instance either of Dr. Smyth or his patrons, with the rudeness of an opponent, or the malignity of a rival; I supposed him to be a man of sense and experience, and I confided in him as a man of candour and honour, who, without undervaluing
the

the discovery of my father, would be satisfied with the praise, which really, and under all circumstances, was due to himself.

Now my father's health was rapidly declining, when Dr. J. C. Smyth's petition was presented to the House of Commons, in February, 1802. He died in April, before the committee, to whom the petition was referred, made their report. Believing the prayer of the petition (so far as Dr. Smyth's discovery of the power of mineral acids in the state of gas to destroy contagion was concerned) to be erroneous, I wrote upon the spur of the occasion to Sir William Pulteney, because he was the old friend of my father, and to Mr. Addington, because his approbation in parliamentary business was essentially necessary. I stated to them in general terms, my father's claim to prior discovery. The minister did me the honour to acknowledge the receipt of my letter, by his secretary, Mr. Barton, and it was the
advice

advice of Sir William, that I should write to Mr. Wilberforce, who had presented Dr. Smyth's petition to the House of Commons. To this gentleman I accordingly wrote, informing him of my father's claim, and at the same time of my desire not to interfere with any proper claim of Dr. Smyth's. The following is a copy of Mr. Wilberforce's obliging answer :

House of Commons, March 23, 1802.

SIR,

I am ashamed and concerned to think that I have treated you with incivility, in so long delaying to answer your polite letter. Nothing, I can truly declare, has been further from my intention ; but having been exceedingly occupied for some days, and having deferred for a little time the subject of Dr. J. C. Smyth's petition, it really escaped my recollection that I had not written to you.

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Inow

I now beg leave to return you my thanks for your obliging communication, and am indebted to you for favouring me with any information to assist me in the discharge of my parliamentary duty. In presenting Dr. C. Smyth's petition, I was actuated not by personal motives, but by a sense of public duty. Though certainly I had a strong presumption, I would not term it a full conviction of the truth, or rather accuracy of the statements in Dr. Smyth's petition, that the nitrous fumigation as advised by him extinguishes contagion, that it may be used without inconvenience to the sick, in their chambers, and that it was Dr. Smyth's discovery. The committee will be desirous of fairly investigating the subject, and if you can state any facts or circumstances which you think material for their information, I shall be much obliged if you will make me acquainted with them, and I will lay them before the committee.

It is my wish to examine fully, and to form an impartial judgment in the result of it. Dr. Johnstone's character, I assure you without a compliment, gives too much weight to whatever comes from him, not to make me desirous, on every ground, of receiving any elucidation he can give to any subject. I am much pressed for time at this moment, and am writing at a committee-table amid interruption; I will only therefore add, that the committee on Dr. C. Smyth's petition will sit on Tuesday next, before which day, any communication with which you may favour me, will be in full time for being produce before them.

I am with respect, Sir,

Your most obedient Servant,

(Signed)

W. WILBERFORCE

In compliance with the desire expressed in this letter, I sent the testimony of my brother

Dr. Edward Johnstone, that he had proposed the use of muriatic acid gas to destroy contagion, in the Royal Infirmary at Edinburgh; and that he defended its usefulness before the professors of that university in 1779. I sent also my own testimony, that I had known it used in the malignant fever of my brother in 1776. At the same time, my father's Dissertation on Fever, and my brother's Treatise on Malignant Angina, were sent to Sir W. Pulteney, in order to be presented to the committee. From Dr. Smyth's letter, I find that they were presented. Here the matter rested. My father died in the course of the enquiry, and the hurry and anxiety occasioned by his sickness and death prevented me from taking any other steps at that time.

Entirely unacquainted with the proceedings of Parliament in such cases, I did not for some time know that any report on the merits of Dr. J. C. Smyth's petition had been printed.

printed. But having casually learnt from Sir *John Wrottesley*, that there was such a report, I procured it from him in August 1802. The tenor of this report convinced me, that my father's claim had not been understood, and I therefore made the comparative experiments on the respective convenience of application of the nitric and muriatic acids in a state of gas; I farther obtained the evidence of professional men, and of other men of science; and I wrote the pamphlet entitled, "Account of the Discovery of the Power of Mineral Acids in a State of Gas to destroy Contagion." Such is the history of my pamphlet.

It was not to wrest from Dr. Smyth the reward which he so well deserved for his successful application of *one* of the mineral acid gases on board the Union hospital-ship, that I made these attempts. Had his reward been ten times greater I should have rejoiced, provided
that

that reward had been assigned to him, not for having primarily discovered the general principle, but for having successfully employed one particular species of mineral acid. For in truth he was not the discoverer. The real discoverer was my father. His claim therefore I asserted in my pamphlet. I endeavoured to assert it with firmness, but without violence; I meant to prove, and not to declaim. In doing justice to the merits of my father, I did wish, and I did strive, not to interfere with the fair fame of other men. It was my earnest desire that the matter in dispute should be decided by reason and the force of facts, and should not be disturbed and obscured by any of the angry passions. I flattered myself that I had succeeded, for my pamphlet was seen and approved, before its publication, by men venerable from their age, their situation, and their talents; the sanctity of whose character, and whose profound knowledge of life

and

and of science, will at least afford me shelter from wanton reproach.

By Sir William Pulteney's advice, I had prepared my account as a paper for the Royal Society in the first instance, and had sent it for his perusal, that it might afterwards be presented to that learned body by their very celebrated president, Sir Joseph Banks. The following letters determined me to publish it as a separate treatise.

London, 30th March, 1803.

SIR,

I received yours of the 22d two days ago, and have read with great satisfaction the paper you sent me on the comparative merit of your father's discovery of the muriatic acid for destroying contagion, and of nitric acid for the same purpose. I have forwarded it with your letter to Sir Joseph Banks for the Royal Society, and I should think that even if it should be published, as I trust it will, by
the

the Society, yet that it ought also to be published as a separate treatise, not only for the honor of your father's memory, but for giving very important information to mankind on a subject of very great importance.

I approve very much of the style and manner in which you have detailed the particulars concerning this matter.

I am, &c.

(Signed) WILLIAM PULTENEY.

(Directed)

Dr. John Johnstone,
Birmingham.

Soho-square, April 4, 1803.

SIR,

The paper you forwarded to me relative to your father's claims on the public, as the real inventor of mineral acid fumigation, has come safe to my hands ; it is ably drawn up, and appears to me very proper for the public eye on many accounts.

It

It does not however appear to me from its nature a proper communication for the Royal Society, or one that the committee of publication would easily be induced to print in the Philosophical Transactions; it states little, if any thing, new, is principally controversial, and would certainly draw forth from Dr. Smith an answer, which, if the original is published by the Royal Society, he would with much reason expect to be also inserted in the Philosophical Transactions, from the pages of which we wish as far as possible to banish that kind of controversy which turns on claims to originality of invention; besides, Sir, it may be said to be purely medical, and therefore far more proper for the countenance of some of the respectable medical societies that are now formed in London, than for the Royal Society, who have not for many years inserted medical matters in their works.

I shall therefore, Sir, hold myself in rea-

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diness

dinefs to return your book in any manner you may choofe to point out to me, or to deliver it to any perfon whom you may fend to me to take charge of it; and I fincerely hope you will not delay to publifh it as a pamphlet, or communication to fome medical fociety.

I find by converfation with medical men here, that mineral acids have been long ufed as fumigations in apartments liable to contagion, efpecially in the Lock Hofpital; but I do not hear of any inftances fo early as your father's ufage of it at Kidderminfter. I meet alfo with a variety of opinions refpecting the facility of breathing muriatic vapour; on the whole I think a preference feems to be given to the nitric, though by no means fo decifive a one as was affumed by the Committee of the Houfe of Commons in Dr. C. Smyth's cafe.

I am, &c. (Signed) JOS. BANKS.

Dr. Johnftone.

My

My late highly esteemed and learned friend Dr. Percival of Manchester, to whom also I sent my pamphlet in manuscript, returned it with the following observations: "I am
 " ashamed of having so long delayed to return
 " your valuable and interesting manuscript,
 " which I have read with attention and with
 " peculiar satisfaction. You have vindicated
 " the claim of your late excellent father to a
 " very important discovery, in a manner which
 " does honour to your abilities, candour, and
 " filial piety; and I am confident that the
 " public will approve your exertions on this
 " occasion, and that even my friend, Dr. Car-
 " michael Smyth, will find in what you have
 " done, no cause of umbrage or complaint."
 He was mistaken. In 1805 came out Dr. Smyth's letter to Mr. Wilberforce, in answer to my pamphlet published in 1803.

I shall now go over the ground again of my former statement, with the addition of fresh

evidence, and such elucidations as Dr. Smyth's letter seems to me to require, to justify my assertion, *that Dr. James Johnstone, sen. my father, had, in the year 1756, discovered a method of destroying contagion, that could be used with perfect convenience in the apartments of the sick.* The main evidence of the date of the discovery is contained in *An historical Dissertation concerning the malignant epidemical Fever of 1756, &c. by James Johnstone, M. D. London printed, 1758.*

“ I have ventured,” (says my father in the preface) “ to make this historical essay public, “ upon a presumption that the observations it “ contains may be usefully applied in treating “ fevers of the malignant class. It has been “ my custom since I have engaged in the practice of medicine, to write down short memorandums of the cases of most of my patients labouring under fevers and other diseases, that I might have it in my power to “ consider

“ consider those which were most remarkable
 “ at my leisure. From a great number of such
 “ cases the descriptions and observations in
 “ the following tract are deduced and com-
 “ piled. In treating of the cure of malignant
 “ fevers, *I lay down no sanative precept which*
 “ *I have not, in my sphere of practice, expe-*
 “ *rienced abundantly useful, and generally suc-*
 “ *cessful.* The cautions which I enter against
 “ known practices or remedies are also drawn
 “ from a repeated observation of their mis-
 “ chievous effects. Some theoretical reason-
 “ ing and inferences, however unfuitable to an
 “ historical piece, I have added, with a view
 “ of rendering the nature of malignant disor-
 “ ders more generally understood than, per-
 “ haps, they are by many who will be under
 “ the necessity of treating them; and also,
 “ further to recommend those rules, of the
 “ utility of which *I was primarily convinced*
 “ *by observation,* to those who demand evidence
 “ of

“ of a different nature. Many practitioners
 “ love to know *that a peculiarity in practice is*
 “ *rational, as well as in fact successful*, before
 “ they make it their own. In a word, I have
 “ thrown together in a narrow compass, and
 “ with all possible plainness, several important
 “ experimental truths, which I hope may be
 “ of service in preparing many, successfully
 “ to combat these terrible disorders in situa-
 “ tions and circumstances similar to those I
 “ have described.”

Such are the declarations of my father
 concerning his own meaning, and his own
 work; a work which he avows contains
peculiarity in practice founded on his own
 experience, and in which he lays down
no sanative precept unwarranted by its utility
 and its success in his own practice. In the
 fifteen first pages of the dissertation an account
 is given of the chief diseases that prevailed in
 the borough of Kidderminster from 1752 to

1756.

1756. At p. 17, speaking of the malignant fever of 1756, "The fever which prevailed during this remarkable year, was very evidently contagious, for whole families were either altogether, or one after another, seized with it. Those that visited or nursed the sick in one house often carried the distemper along with them to other places, and were seized with it themselves." From p. 20 to 26 the symptoms of this fever are detailed at length. "In every degree of this disorder lowness, debility, restlessness, nausea, head-ach, general propensity to coma or delirium," were its symptoms, and in the worst cases "black parched tongue, flat livid petechial spots, and a quick low soft pulse." The prognosis to p. 32; from thence to p. 51, the method of cure. Among other methods, at p. 41 the dulcified acids and the anodyne mineral liquor of Hoffman are recommended. "At the beginning of the fever the spirits of
" nitre,

“ nitre, frequently taken by tea-spoonfuls in
 “ mint tea or the patient’s common drink,
 “ will answer every purpose, and may alone
 “ be depended upon,” p. 43. “ At the
 “ height of this fever then larger doses of bark
 “ in substance, with elixir of vitriol and red
 “ wine, were highly useful and necessary,” p.
 45. Towards the decline, when there were
 white specks or sloughs about the glands or
 palate, resembling those of ulcerated sore
 throats, “ these were treated with antiseptic
 “ detergent gargles of tinct. myrrh. spir. of
 “ vitriol, oxymel simp. and the bark in sub-
 “ stance was given internally, with vitriol, in
 “ larger or more frequent doses than in any
 “ other condition of this disease.” p. 48, 49.
 In the next page comes the consideration of
 the air, in which is included the first notice
 of the use of the mineral acids in a state of
 gas to correct contagion. “ The tempera-
 “ ture of the air in which the patient breathes
 “ must

“ must be rendered as favourable to his re-
 “ covery as possible, throughout the whole
 “ course of the disease; for this reason it must
 “ be continually renewed, especially when the
 “ patient or patients are crowded in small
 “ confined places, by opening the doors and
 “ windows of the apartments, so as to admit
 “ the free passage of the air. This practice,
 “ so necessary in the small pox, is no less so in
 “ every species of putrid fever; but it must
 “ be done so as not to repress perspiration.
 “ The necessity of changing the air in a sick
 “ room, by successive ventilation, arises from
 “ the constant destruction of a certain pro-
 “ perty in that fluid by breathing, which ren-
 “ ders it afterwards useless; likewise from the
 “ atmosphere being filled with the excremen-
 “ titious steams which fly off from the pa-
 “ tient’s body continually, and which putrify
 “ in a stagnant unrenewed air, and render
 “ it truly poisonous, a pabulum morbi, ra-

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“ ther

“ther than of life. The physician will order
 “the room to be kept sweet and clean, and
 “fools and every thing offensive to be removed
 “as soon as possible. If the external air is
 “immoderately cold and wet, the room must
 “be kept warm and dry, and the fumes of
 “amber, myrrh, benzoin and camphire, may
 “be diffused in the room, if sprinkled upon
 “hot-iron. Vinegar may be sprinkled about
 “cold, if the weather is warm, and boiled
 “with myrrh or camphire, an antiseptic
 “steam will arise into the air, which the pa-
 “tient breathes greatly to his advantage.
 “These steams will preserve the air free from
 “putrefaction, and will insinuate themselves
 “by the absorbent vessels of the lungs into the
 “blood-vessels, and will greatly assist in im-
 “peding the progress of putrefaction in the
 “fluids. These are the most commodious,
 “if not the most useful, methods of medicat-
 “ing the air the patient breathes; however,
 “those

" those who prefer the mineral acids may
 " order brimstone to be burnt, or may raise
 " the marine acid very easily, by putting a
 " certain quantity of common salt into a ves-
 " sel kept heated, upon a chafing-dish of
 " coals ; if to this a small quantity of oil of
 " vitriol is, from time to time, added, the air
 " will be filled with a thick white acid steam ;
 " but both the marine and sulphureous acids
 " must be disengaged at a considerable dis-
 " tance from the patient, otherwise their ex-
 " treme pungency will be offensive to the
 " lungs." p. 50, 51. At p. 53, 54, are some
 personal directions for escaping contagion ; the
 rest of the dissertation consists of theoretical
 reasoning on putrefaction, and some general
 remarks and deductions.

Such is my father's work. I have analysed
 it as shortly as is consistent with a clear enu-
 meration of the contents ; and from these
 contents I confidently appeal to all men of

science whether it be not indisputable that my father not only admitted and understood, but had distinctly observed and actually experienced the power of the mineral acids in a state of gas, as correctors of putrefaction, in 1756. In his preface he says, "I lay down no fanative precept which I have not, in my sphere of practice, experienced abundantly useful." At p. 51 he lays down the fanative precept to raise the mineral acids, and gives precise directions, so far as relates to the muriatic acid gas; and yet, after quoting the whole passage at p. 51 of the Historical Dissertation, Dr. Smyth says, at p. 7 of his Letter to Mr. Wilberforce, "The preceding passage requires little comment, a fair and candid exposition of the text being all that is necessary to do away the pretended claim. Dr. Johnstone recommends the fumes of amber, benzoin, &c. with the fumes of vinegar, as the effectual means of resisting putrefaction

" or

“ or destroying contagion ; and only mentions
 “ the mineral acids of sulphur and sea salt
 “ upon a supposition that some persons might
 “ choose to employ them. For such persons
 “ he gives the common directions how they
 “ are to be procured, and a caution respecting
 “ the inconvenience or danger that may follow
 “ from their use ; although his directions on
 “ this subject are too general and inaccurate
 “ to be of much service.”

In the first paragraph Dr. Smyth talks of “ *pre-
 tended claim* ; ” — in the last he acknowledges
 that “ *common directions*,” how they are to be
 procured, and a caution respecting the inconve-
 nience or dangers that may follow from the use
 of mineral acid vapours, are given. By what al-
 chemy of reasoning Dr. Smyth can reconcile his
 assertion of pretended claim with the subsequent
 concession of common directions, is to me ut-
 terly obscure. My father's own words may
 convince the meanest capacity that, in 1756,

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he used the muriatic acid gas for the purpose of keeping the air free from putrefaction. Now, my position is, that he did use this gas at that time, for that purpose. Does Dr. Smyth mean, by pretended claim, to say that my father's words admit of no such explanation? That marine acid does not mean marine acid, and that to keep the air free from putrefaction is destitute of any allusion to the general signification of those words? If so, he contradicts himself by conceding that my father has given common directions. Can he pretend to say that the printer has inserted a sentence in my father's book contrary to my father's intention, and which my father did not understand? If these be the grounds of his assertion, "pretended claim," let him deduce the corollary from the problem himself. Of pretended claims, indeed, the kinds are many and various, and Dr. Smyth may conceive the nature of some of them without any violent

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lent stretch of the powers of his imagination. It is possible, for instance, that men may pretend they make a discovery in 1780, after deliberate reflection, when the same discovery is to be found in obvious places long before. They may pretend that they have discovered a principle in 1795, when the slightest investigation may convince the reader that they have only varied the agent. In fine, they may claim reward in 1802 for discovery; which discovery, when they ask reward in * 1781, they never specify, although the deliberate reflection that led to such discovery is said to have taken place at the remote period of 1780.

At p. 8 of his Letter Dr. Smyth quotes a passage from my pamphlet, in which I have asserted, "that near fifty years before the framing of the report of the committee of

* See Dr. Smyth's memorial to Lord North.

"the

“ the House of Commons, a country physician
 “ (my father) had acquired eminence by the
 “ discovery of a certain method of destroying
 “ contagion, which could be used with perfect
 “ convenience in the apartments of the sick.”

I adopted these terms, because in the report of the committee a sentiment of Dr. Lind had been quoted nearly in the same words, and because they formed the basis of the result of the report, as a document proving that no discovery of a power to destroy contagion had been made before Dr. Smyth's. For if any other person had discovered a certain method of destroying contagion *before* Dr. Smyth, then could not *he* be the discoverer. Now in the second paragraph I before quoted from Dr. Smyth's Letter, he does acknowledge that my father gives “ the common directions how
 “ they (mineral acid vapours) are to be pro-
 “ cured, and a caution respecting the inconve-
 “ nience

“ nience or danger that may follow from their
 “ use.”

Did any one ever give similar directions before? No. Was the muriatic acid gas ever directed for the curative purposes of medicine before? No. Were these directions *common* when my father gave them? No. When, where, and for what purpose, were these directions, which I grant to be no common, given by my father? The time was 1756; the place was Kidderminster; the purpose was to cure a malignant fever, and the book in which that purpose is announced appeared in 1758. Yet, with a strange inattention to these plain facts, Dr. Smyth goes on, triumphantly declaring, that from my father's book, “ no inference can be drawn that he
 “ *ever* employed the mineral acids of sulphur
 “ and sea salt; and that so far is he from re-
 “ commending them to others, he cautions
 “ us against the mischief they may occasion.”

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These are Dr. Smyth's words, and the obvious import of them is, that my father *only* cautions, and by caution meant tacitly to discourage, rather than to recommend. Let us particularly attend to this specimen of Dr. Smyth's logical talents. Perhaps the Doctor may have heard of certain wise sayings very generally adopted in the language and the commerce of life, all of which indicate the necessity of caution. Perhaps, too, his own personal experience may have convinced him of the utility of some of them, for the conduct of life. If so, he must have discerned that there is a caution in selecting as well as rejecting, and that too much or too little, are as frequently the subjects of it, as decided and absolute choice. My father in his practice as a physician, had doubtless learnt the necessity of caution. He had found it, doubtless, necessary to limit the doses of medicines, and to regulate their application. In the appli-
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cation of a *new* and powerful medicament he was especially called upon to exercise this caution, and he therefore advises the acid vapours “to be disengaged at a considerable distance from the patient, otherwise their extreme pungency will be offensive to the lungs.” And from these terms does Dr. Smyth infer, “so far is he from recommending them to others, that he cautions us against the mischief they may occasion;” thus misconstruing a salutary caution into rejection, confounding the use of a medicine with its abuse, and strangely inferring ignorance, from that very circumstance of discrimination, which is usually considered as the criterion of attentive observation and accurate knowledge. How *could* my father caution against their extreme pungency had he not used them, and attended to their effects when used? Dr. Smyth cannot be ignorant that there are salutary doses of muriatic acid gas, as of all other

medicaments, and that it may be raised in a quantity so small, as to be inefficacious, or so great as to be suffocating. The medium is the salutary dose, that which my father's experience had taught him could be administered with perfect convenience, and which he virtually recommends, in guarding against a too pungent and irritating quantity. The precept which he gives therefore, for raising it at a distance from the bed of the patient, so far from affording any colour of probability to Dr. Smyth's language, "no inference can be made," is particularly wise and useful, and shews that my father gave this caution, as I contend, experimentally, and, as Dr. Smyth must admit, justly; for the gas is thereby diluted before it reaches the patient, and has none of those irritating effects on the organs of respiration which it would have if inspired unmixed with a portion of atmospherical air. But how could my father know this, unless he
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had seen marine acid used, sometimes well, and sometimes ill? Let me remind Dr. Smyth that Mr. Menzies, in his first trials of nitric acid gas on board the Union hospital ship, raised it too near the faces of the patients and thereby excited coughing*. Had Dr. Smyth's experience instructed him in the same cautious use of the nitric acid gas which my father's experience had taught him of the muriatic, he ought to have mentioned the caution, and then Mr. Menzies' practice would at first, have been safer. But this essential knowledge Mr. Menzies was left to acquire from his own experience, as he probably had never seen the caution of my father, and as he was left uninformed by Dr. Smyth. Knowing that the caution of my father was important as well as just, and that it was equally applicable to the nitric and muriatic acids, I expected to find

* Smyth's Account of the Experiment, &c. p. 12.

some mention of it in the full and elaborate account which Dr. Smyth has given of his treatment of the sick at Winchester. If the nitric acid gas was really employed for them, Dr. Smyth could not fail to observe the same effect which my father had observed with the muriatic; and it is somewhat singular, that if observed by Dr. Smyth, it is not recorded in a statement which no medical man can read without instruction, and which does the highest credit to the diligence, sagacity, courage, and benevolence, of Dr. Smyth. I am content with noticing the omission, and shall leave Dr. Smyth at his leisure, and according to his discretion, to account for it. But to proceed.—It is nugatory, and almost captious to assert, that my father never used the muriatic acid gas, and that he makes no distinction between the sulphureous and muriatic acids, when in the passage at p. 51 of the Historical Dissertation, he gives precise directions

tions only for raising the muriatic. Without experience, he could not know that oil of vitriol poured on common salt would fill the air with a thick white acid steam *, and that this vapour was an useful method of medicating the air, and of preserving it free from putrefaction. It is true that my father recommends vinegar imbued with many odorous gum-resins, and his Dissertation would have been

* This was not the chemical knowledge of 1756: even in Dr. Priestley's first experiments on air he had not hit upon this method of procuring the muriatic acid gas. In his "Experiments and Observations on different Kinds of Air," published 1774, he says, p. 229, § iv. "of Acid Air:"

"In my former experiments on this species of air, I procured it from spirit of salt; I have since hit upon a much less expensive method of getting it, by having recourse to the processes by which the spirit of salt itself is originally made. For this purpose I fill a small phial with common salt; pour upon it a small quantity of concentrated oil of vitriol, &c." which was in fact my father's method, and used long before Dr. Priestley's.

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imperfect without the mention of them, for medical knowledge was not then so matured as to warrant the rejecting them altogether ; but his words obviously imply a clear knowledge and a strong approbation of the mineral acids as useful agents, if employed with caution, although in this, as in many other parts of the Dissertation, the phraseology might have been chosen to greater advantage.

As to vinegar* he certainly ought not to have given it up as an useful agent, well knowing that the practice of physic requires a variety of agents, and that those of weaker powers are sometimes more commodious in application, and therefore, in certain cases and situations, preferable to the most active. His experience must soon have convinced him of the

* The experiments of Guyton de Morveau prove that vinegar is not destitute of the power of destroying putrefaction, although less efficacious than the mineral acids. *Traité des Moyens de Désinfecter l'Air*, p. 145, et Exp. 18, 19, 20, 21.

disagreeable-

disagreeableness of the sulphureous acid gas, and of the impossibility of burning brimstone in any considerable quantity in inhabited rooms. But it is equally efficacious in disinfecting those that are uninhabited; and therefore, on account of the principle, my father mentions the sulphureous and marine acids together. Let it be remembered too, that brimstone was one of the fumigations used by the old physicians, but not recommended by them as affording a mineral acid when burnt, whereas my father recommends it, because when burnt, it *does* afford a mineral acid, which mineral acid in a state of gas destroys contagion.

Before Dr. Smyth infers that my father considered the marine and sulphureous acids as equally safe and equally useful, in the first place he must admit that my father had tried both; and secondly, he must contend that he was too stupid to see that difference

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in the result of the trial which every patient must have experienced, and every nurse must have observed, if the trial had been made without any regard to proportion in the quantity. He says that brimstone may be burnt, and Dr. Smyth knows as well as I, that it often had been burnt by the old physicians, for the purpose of driving out contagion. But my father does more. He says it may be burnt for the purpose of medicating the air, that is, correcting contagion, and this power he ascribes to brimstone, *only when converted into a mineral acid*. I will grant to Dr. Smyth that my father would have written more satisfactorily, and more usefully, if he had written more copiously and more explicitly, upon the comparative qualities both of the sulphureous and muriatic acids. But I contend that he was acquainted in 1756 with their proper and specific power of destroying contagion; with
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that power which they contained as mineral acids, the very term be it observed, by which my father describes them.

At p. 9 of his letter, Dr. Smyth proceeds thus: " Having laid before you the passage
 " in the father's book, and pointed out the
 " only conclusion that can be drawn from it,
 " I shall in the next place examine the account, or rather misrepresentation, of his son,
 " Dr. John Johnstone. After mentioning the
 " title of the father's Dissertation, published in
 " 1758, he proceeds: ' In that Dissertation,
 " adopting the theory of the day, he proposes
 " to keep the air free from putrefaction by the
 " steams of vinegar, or, as a more effectual
 " method, the marine acid may be raised very
 " easily, &c.' " Let it be observed, that in the first part of the sentence above quoted from my pamphlet, I merely declare the meaning of my father; I do not quote his words till I come to the phrase " the marine acid, &c."

I believe I did not err in stating that my father, by recommending vinegar, adopted the theory of the day—the theory of Fothergill and of Huxham, which Dr. Smyth has so archly marked in *Italics*. And that he proposed in the mineral acid gases a method which is in fact more effectual, can any man have the effrontery to deny?

Enough has already been said on my father's caution respecting the use of mineral acid vapours, and I trust it is clear, that so far from "flatly contradicting my assertion," (that the method of destroying contagion, discovered by my father, could be used with perfect convenience in the apartments of the sick,) as Dr. S. chooses to express himself, this caution proves my assertion. I trust it is also proved that my father's terms *do* convey his conviction of the efficacy of the mineral acids in a state of gas, to destroy contagion; and whether, according to the strong language
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of Dr. Smyth, my account is "such a complete perversion of an author's meaning, professing merely to be a declaration of it, (as) has never before been presented to the public," must be determined on a fair and accurate comparison, by an unimpeached and impartial judge.

It will be immediately seen, by those who may take the trouble to compare the two passages in the two pamphlets, that in Dr. Smyth's letter to Mr. Wilberforce the whole of the passage beginning at the words, "In that Dissertation," is printed with inverted commas, without any distinction of parts, and yet upon the whole as a quotation, interpolated by me, in order to pervert my author's meaning, Dr. Smyth criticises arrogantly and contumeliously. Violence is seldom the criterion of a just cause, and at least it behoves a man to be correct before he become violent. By inspecting page 5 of my pamphlet,

pamphlet, it may seem that the words "more effectual method," are not quoted by me as the words of my father's book, and are not, therefore, an interpolation. They are my own words, and I quote them as such, and as I conceive, at the same time, declaratory of my father's meaning. I do not quote my father's words till inverted commas are placed at "the marine acid, &c." Instead then of my father's meaning being misrepresented by me, Dr. Smyth has misrepresented mine. He first makes me quote that which I do not, and then derides me for the blunder which he himself has made me commit. My father's words, thus misquoted by Dr. Smyth, were intended by me to be declaratory of my father's meaning, and I still think myself justified in thus interpreting the passage "these are the most commodious, if not the most useful, methods of medicating the air the patient breathes; however, those who prefer the mineral acids, &c."

“&c.”* The vinegar is the most commodious method doubtless, but if it be not the most useful, what is? There can be but one opinion, the mineral acids. The language is imperfect I grant, and in an interleaved copy of his Dissertation my father has himself inserted the words “some of,” before “the

* I have shewn these words to several persons of the learned professions, and they thought as I did, that in my father's opinion they implied a preference of the marine acids to medicated vinegar. I have shewn them also to another friend, who is not very likely to be cajoled by sophistry, or dismayed by dogmatism; but that friend, with all his habitual admiration of my father, and all his strong indignation against the general spirit and general reasoning of Dr. Smyth's pamphlet, declares to me, that in his opinion the words under consideration do not decidedly imply preference. I have not concealed this opinion because it was contrary to my own, and because it did not suit my purpose; nor will I conceal from Dr. Smyth, that my conviction of the sagacity, wisdom, and uprightness of the learned friend from whom I differ in this one instance, generally induces me to repose under his authority, and to submit to his guidance.

“most

“most commodious.” Were I, however, *ex hypothesi*, to concede to Dr. Smyth, that from my father’s words in this place, a reader might be justified in supposing, that the use of vinegar in my father’s opinion, was more commodious if not more useful than the mineral acids, the question would then turn upon my father’s opinion of their *comparative* usefulness, but his knowledge of their *positive* use would still be indisputable. He must yet have known, that the mineral acids had the power of medicating the air, that they who preferred them might use them commodiously and usefully in some degree, be it great or little, and that by being prepared in a certain way, and disengaged at a considerable distance from the patient, both acids would destroy contagion. Thus much is proved by my father’s words, if understood in the most unfavourable sense which an intelligent and impartial critic can assign to them. They justify my assertion

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that my father had discovered a certain method of destroying contagion, which could be used with perfect convenience in the apartments of the sick, by observing his caution as applied to them respecting it. They confute Dr. Smyth's position, that no inference can be made from them, that my father *ever* employed the mineral acids of sulphur and sea salt ; that he is far from recommending them, and is content only with cautioning us against the mischief they may occasion. If my father had found them of no use, he had the fairest opportunity of saying so when he was contrasting their efficacy with that of vinegar ; and he ought to have said so, when he was speaking of the mischief which they might occasion by irritating the lungs. A writer of the most ordinary talents, and the most ordinary humanity, would have said that there was much mischief, but no utility in employing them, and, instead of restraining the mischief by cau-

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tions, he would have averted it by peremptory and direct prohibition. Whatsoever then be the interpretation of my father's meaning, Dr. Smyth's exposition of the text of my father's words is neither fair nor candid, nor sufficient to set aside the claim of discovery, that the mineral acids did possess some power of destroying contagion, and that they could be used with some degree of safety as well as efficacy.

My father then must be confessed to have discovered the principle common to all the mineral acids in a state of gas, that of destroying contagion. He assigns to the sulphureous acid gas a specific power which had never been assigned to it before. He gives directions for raising the muriatic acid gas which had never been given before. He gives cautions respecting the muriatic, which, of course, could not have been given before. He repeats cautions about the sulphureous, which probably had been given before, because the
effect

effect against which that caution is directed must have been observed when brimstone had been burnt for a different purpose than that of destroying contagion. My father's experience, like that of every other intelligent observer, doubtless, enabled him to improve and regulate the use of the muriatic acid gas, after the publication of 1756; and that he continued to use it after that publication, and antecedently to the publication of Dr. Smyth's book in 1795, and even his experiments made, or I should rather say, intended to be made, at Winchester in 1780, will be shewn by me abundantly, before the conclusion of my reply to Dr. Smyth.

I am sorry to have been thus long engaged in a warfare about words—But words are the vehicles of facts, and, as it is my wish to have facts understood, I shall never be ashamed of confessing any real or seeming error about the import of words by which facts are conveyed.

In my endeavours to be correct and ingenuous, I may have been prolix. I have not forgotten that Dr. Smyth has talked in his letter about a short exposition of my father's text. But there is a sort of conciseness which produces obscurity, and there is a sort of obscurity very convenient for writers, who, professing not to weary their readers, are intent upon misguiding them.

I now come to Dr. Smyth's negative proposition, founded upon certain other quotations from my father's dissertation, in the selection of which he has not forgotten the precept of the antient poet and philosopher,

Νᾶφε καὶ μεμνάς ἀπιστεῖν, Ἄεθρα ταῦτα τῶν φρενῶν * :
and which he has put together like a master in the school of the Ephectics and Aporetics †.

* Cic. Ep. ad Attic. I. 19.

† With the common places of suspension, and the ὅτι τῶν πρώτων of Ænefidemus, I will not weary the reader. The scholar finds more of this sort of learning than he can understand in Sextus

The propositions are, "that Dr. Johnstone, the father, neither knew the superior efficacy of the mineral acids in destroying contagion, nor *ever* had the most distant idea of employing them with this intention." When an author is quoted, his opinions ought to be cited in his own order and connection; for by inverting sentences, and detaching the terms from those links which bind them together in a common meaning, it is in the power of any bungler to overturn all sense and verity, and to make even the holy scriptures themselves utter blasphemy and falsehood. Let us now attend to the two extracts from my father's Dissertation, which Dr. Smyth introduces to support his negative proposition. The first is from p. 53 of the Hist. Diff. in which my fa-

Epipiricus; perhaps the learned doctor has sharpened the edge of his subtlety in Pyrrhonic: Hypotypos; lib. 1. cap. 17.

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ther gives personal directions for guarding against contagion. At p. 51 he has directed the means which he thinks most efficacious for medicating the air, that is, destroying the contagion in the apartments of the sick. At p. 53, 54, he recommends those rules which are best calculated to maintain health, and consequently to resist the poison of infection. “ The
 “ great business of preserving the body free
 “ from contagion consists in preserving the
 “ motion of the blood free and regular, and in
 “ supporting insensible perspiration in a regular equable tenor. Fatigue of body and
 “ dissipation of spirits, and an intemperate use
 “ of the non-naturals, must be avoided. Wines
 “ are neither to be used immoderately nor
 “ yet too sparingly. I have seldom known
 “ habitual drinkers of red wine affected with
 “ putrid diseases; this and the acids of lemons, oranges, vinegar, tartar, spirit of
 “ nitre,

“ nitre, and elixir of vitriol, and the bark,
 “ are the best preservative, as well as excellent
 “ curative remedies. The mind must be pre-
 “ served in a fearless exhilarated state. Lastly,
 “ persons who frequent the beds of the sick,
 “ either in visiting or attending upon them,
 “ ought to avoid breathing in the atmosphere
 “ which is contiguous to the patients’ body as
 “ much as possible; the saliva should never
 “ be swallowed in the room; myrrh may be
 “ held or masticated in the mouth; tobacco
 “ smoked; and the mouth may be washed
 “ with vinegar or Tinct. myrrh. in water, after
 “ finishing those offices which duty and hu-
 “ manity require to be performed to the af-
 “ flicted.” In the first part of this passage the
 directions are purely anticipative and precau-
 tionary; they instruct the man who is exposed
 to contagion, or who dreads it, how he best
 may maintain his constitution in a sound con-
 dition; they relate merely to the manage-
 ment

ment of his exercise, his diet, his phyc, and his passions. In the last part he gives directions to those who frequent the beds of the sick. He has already told you how the sick man is to be treated ; he has already ordered the sick chamber to be fumigated by vinegar or by mineral acids, to destroy contagion ; avoid, however, breathing too near the patients' body, avoid swallowing your saliva in the room, and *after finishing* those offices which duty and humanity require, wash &c. with vinegar. When Dr. Smyth cites the passage from p. 54, he entirely forgets or neglects the concluding sentence, " after finishing ;" he chooses not to notice that the directions are in the first place, personal and precautionary, and that to recommend the use of muriatic acid fumigation *before* a man enters the sick chamber is not of the highest and most cogent necessity. While he is *in* the chamber he has all the benefit of the acid fumigations,

fumigations, for *there* my father shews how they may be raised, and *after* finishing those offices which duty and humanity require, that is, after the friend, the nurse, or the physician, is got home, it would be superfluous to raise them. The mention of the sulphureous or marine acids would have been still more superfluous at p. 17, for there my father merely gives a history of the disorder, and of its contagious nature. To have mentioned them at this place would have been utterly irreconcilable with the method of his composition; they would have been out of place. Methods of cure and precaution are inserted at p. 51, 53, 54, of the Hist. Diff., and yet, after quoting p. 17, which is purely historical detail, comes out Dr. Smyth's fine apologue, in which the negative proposition is summed up with all becoming sentimentality. According to Dr. Smyth's mode of reasoning, unless my father mentioned mineral acid gas,

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at all times, and under all circumstances, in connection with contagion, it must be inferred that he knew nothing of the matter. It is not sufficient evidence that he gives precise directions for preserving the air free from putrefaction, by diffusing the gas of mineral acids in those places where contagion actually prevails, unless he orders them, too, where it does not prevail. He can never have had the most distant idea of employing them with the intention of destroying contagion unless the words are always in his mouth, or under his pen. Let him give "common directions" for raising them before any other physician thought of it, let him use them all his life whenever occasion call for their assistance, it is all a pretended claim, unless their use be urged in every sentence of his writings and his conversation. Like the Indian Fakir, let him intermit his devotion or his penance for a moment, and there is an end of his former merits

rits and his prior claims. Has then Dr. S. himself been this medical Fakir? Have his conversations and his writings been perpetually engaged by the virtues of nitric acid gas? At least his own declarations concerning his own practice entitle us to entertain some doubt, as we shall see hereafter. If those doubts have any foundation, I must leave it to Dr. Smyth to *reconcile the supposition* of his being furnished by his own deliberate reflection, in 1780, with a method of preventing the spread of infection, *with the fact* of his not employing it till 1795. "Humanity revolts at the idea," is the sentiment he applies to my father. Let me recommend him in future, before he weaves such sentiments on such grounds, to look to himself, and before he applies them, to consult the exclamation or the apostrophe of Sir Peter Teazle in the play.

But of all the perverse constructions, and all the hasty assumptions, in which Dr. Smyth

has indulged himself, the most curious are those in which he makes my father responsible for the thesis of my brother James. This inaugural *Thesis de Angina Maligna* was printed at Edinburgh in September, 1773, when my brother took his degree. In the acknowledgments to friends, my brother mentions three names: his father, and his masters Gregory and Cullen. “*Primum omnium, patrem mihi*
“*semper quam plurimum colendum, ut comme-*
“*morem, suadent pietas, amor, officium, animus-*
“*que beneficiorum non immemor, cujus attentio*
“*vere paterna et prudens in me constanter in-*
“*sumpta, multo plus quam primordium vitæ ab*
“*illo derivatum, me illi obligavit, cujusque*
“*consilio amicissimo hocce opusculum, meritum*
“*suum, si quod habeat, præcipue debet.*” *De*
Angina mal. p. 72. Such is the acknowledgment upon which Dr. S. builds his assertion, “that Dr. James Johnstone’s inaugural
“Dissertation is the joint work of himself and
“his

“ his father, that its opinions are a declaration
 “ under Dr. Johnstone’s own hand,” and “ we
 “ can have no doubt that Dr. Johnstone did
 “ bestow considerable pains on this first essay
 “ of his son, which was to be equally honour-
 “ able or discreditable to both.” Behold ano-
 ther glaring instance, in which Dr. Smyth as-
 sumes a position unauthorised by the fact,
 another vigorous scyon, from the sturdy stock
 of Dr. Smyth’s fertile imagination. Here we
 have no less than three distinct assumptions
 branching out from two plain words, the
 meaning of which in plain English, can only
 be construed, friendly counsel. If an honest
 though unlettered man were asked, what
 friendly advice meant, he could be at no loss
 in answering the question for a moment. He
 would tell you that he gave his advice to the
 best of his judgment, under all the circum-
 stances of every particular case, and that he
 did not consider himself as responsible for all
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the consequences that might follow from all the actions of the individuals that asked his advice, and acted upon it ; this would be constituting him an actor, not a counsellor. In the case before us there is not the shadow of evidence, from the acknowledgment above cited, that my father was a joint party in the composition of the thesis. That he gave friendly advice is all that is asserted, the rest is compliment ; and surely, no unjust compliment for a son to pay to his father to say, if there were any merit in what he had done, it was derived from him. He was indebted to him for the first rudiments of education. He owed, and he paid to him, a tribute of gratitude for that paternal attention which, to every enlightened and well-regulated mind, has more endearments than the recollection of existence itself. He had received from him many encouragements to industry, many hints in physic, many lessons in philosophy, which
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formed the basis of a character of no common eminence. In his first public performance, an acknowledgment of these invaluable benefits was due to his father, and he made it. He does nothing more; and it required the full exercise even of Dr. Smyth's talent for discovery to discern that any thing more was intended. It will require more than all his learning to prove that *consilium amicissimum* has the acceptation he has given to it. Will he venture to affirm that the meaning of *consilium* can ever be distorted into a declaration of the same opinion being adopted in the same words, under the same hand? Where is the authority? Counsel must always be a mixed operation of the mind, the result of deliberation on the whole circumstances of the case under consideration. In giving counsel, a wise man will not only consider that which is right, but also that which is expedient. Time, place, motives, age, must all modify his opinions,

nions, to render his counsel profitable and fit for use. In the present instance, a father, who has made an interesting discovery, gives friendly counsel to his son, then in the prime of youth, who is writing a public exercise for his degree. In his public exercise, the son does not mention this interesting discovery; and therefore, thirty years afterwards, when both the parties are dead, it is denied by an interested controversialist, that the father made this discovery. Such is Dr. Smyth's position. I know not whether this gentleman has sons who have published academical exercises, or if they have, whether in such exercises they have mentioned their father's very laudable use of nitric acid gas. Whatsoever be the fact, I should esteem it as feeble in reason and argument, as in resource and design, to assert, that because they do not mention it, he never used it. Nor will I impute to Dr. Smyth either the honour or the dishonour of being a party in the composition

sition of these exercises, although his sons shall extol him according to his merits, or beyond them.

An inaugural thesis can seldom be elaborate in style, or profound in science. It generally consists of a sketch or history of a disease, or is an essay upon some moral or physical subject. It is never intended for the discussion of deep and disputed points in doctrine, and when such discussion is attempted, it is not always thought a proof of good judgment. As the work of a young and inexperienced man it is rather expected to be a clear description and a correct composition, than a didactic or practical work. I can readily conceive, therefore, why my brother should omit the mention of my father's practice; that he knew it soon after this time I shall immediately bring proof. The history of the composition of my brother's thesis is, that it was composed whilst he was absent from my father, at Edinburgh, in the summer of 1773.

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The Latin composition was first prepared by my brother, and then inspected, not by my father*, but by one of my brother's friends at Edinburgh, probably by Dr. Henry the historian, my father's old instructor. My father did not see it till after its publication. When he did see it, he did not suffer his vanity to get the better of his judgment; nor did he reproach his son for not introducing a circumstance which, however creditable to his own reputation, was unnecessary, and might have been considered as impertinent and ostentatious, from the very circumstance of singularity in novelty, in the thesis of a young

* If Dr. Smyth had exercised his talent for conjecture upon my brother's book rather than his thesis he would have been more fortunate, and in my estimation, as well as his own, more sagacious. My father had no concern at all with the thesis; but in the book he certainly did assist my brother, vid. Green's History of Worcester, Vol. 2, p. 69. It is not very probable that an antiquary should explore a medical book, and the plain fact is that my father furnished Mr. Green with the foregoing passage, and with other *particulars* relating to my brother.

man.

man. But what my brother omitted when the propriety of introducing it was doubtful, he did not fail to introduce properly, and to state unequivocally, when his knowledge was more enlarged, when his judgment was more matured, and when, for his opinions and attainments as a physician, he was more directly and more extensively responsible to all professional men.

In the note at p. 25 of his Letter, Dr. Smyth condescends to notice a volume of Medical Essays, containing one Essay of mine "on Mineral Poisons," in which, says Dr. S. "although he mentions particularly the three "mineral acids and their gases, there is not the "most distant hint of their application to de- "stroy contagion." When the heat engendered by the intemperance of controversy has abated, Dr. Smyth will perhaps *discover*, that in a work professedly on poisons, I was not immediately called upon to detail the curative application of medicaments. In regard to the

utility of the Essays, in their present form, I am neither daftardly enough to be terrified by the decision of Dr. Smyth, nor vain enough to offer any opinion of my own. But I can readily spare the increase of credit which might have accrued to my brother or to myself, by the introduction of matter which had no immediate connection with my subject; and I beg leave to assure Dr. Smyth that neither his advice nor his example will induce me to mention my father's discovery, or his own, in any future publication, unless the introduction of them shall in my judgment, be useful, as proofs or illustrations of the facts which I relate, and the principles which I defend.

Because in this one Essay there is no hint of mine on the application of mineral acids and their gases to destroy contagion, Dr. Smyth concludes, with his usual fairness, that the very idea of their power to destroy it did not exist in my mind. But the conclusion is at a wide distance

distance from the premises. The idea must have existed in my mind unless Dr. Smyth will extend his reproaches from my want of judgment to my want of memory. The *Essays* were published in 1795. Now, it so happens that from my earliest youth, and in the vacations during my academical studies at Edinburgh and at Oxford, I frequently visited my father; that for one year, from 1793 to 1794, I assisted him in his medical practice at Worcester; that during this long period I had opportunities not only of hearing my father's opinions on the power of mineral acids, but of observing his success in using them; that I had read my father's book published in 1758, and my brother's book published in 1779. With means therefore so abundant, and so constantly at hand, I did not need the aid of Dr. Smyth's book to form some idea of the power of the mineral acids to destroy contagion. The idea had existed in my mind long before the publication of the *Essays*, and the only addition
to

to my knowledge, for which I am indebted to Dr. Smyth, is, that the nitric acid gas may be used with nearly as much convenience as the muriatic, and with similar efficacy. But even Dr. Smyth's use of nitric acid was not, at that time, known to me. If known I should not have noticed it in my Essay on Poisons, how much soever I might have been convinced of its utility. Let me here correct a small error into which Dr. Smyth has, somehow or other, been precipitated. The Doctor says in his note, that the Essays are chiefly the production of my brother; but if Dr. Smyth's reflections upon his own discoveries will afford him leisure to look once more into this volume, he will find that not *one* of them is written by my brother James. They are all the production of my father, except the cases furnished by Mr. Gomery and by my brother, Dr. Edward Johnstone, and one Essay of mine.

That Dr. Smyth may not imagine the power of muriatic acid gas unknown to my
brother

brother James about the time of his taking his degree, I shall now insert part of a paper written by himself, on the medical virtues of airs, which paper was sent by my father to Dr. Priestley, to be presented to the Royal Society. Dr. Priestley's answer to this communication is preserved; and as it will correct some of Dr. Smyth's visions and theories at p. 21, 22, 23, of his Letter, clear up his doubts, and do away the effect of his cavils, I shall insert it. The paper, written in my brother James's own hand, is entitled,

“Remarks upon the Medical Virtues of
“different Kinds of Air,” by James John-
stone, jun. M. D. dated Worcester, May 24,
1775.

After mentioning, in five pages, different airs which are antiseptic, he goes on as follows :

“Another grand restorative of vitiated air
“of Dr. Priestley is acid air, procured from
“sea salt. This is capable of a very important
“medical

“ medical application, not mentioned by the
 “ doctor and his medical correspondents;
 “ though his experiments and deductions shew
 “ it likely to produce the best effects. *The*
 “ *use I mean is that of correcting the air of*
 “ *jails, hospitals, &c. and medicating the air*
 “ *within the chambers of persons labouring un-*
 “ *der any putrid disease, or such as are troubled*
 “ *with wounds or ulcers tending to putref-*
 “ *cency. This use of acid air was recommended*
 “ *by my father, Dr. Johnstone of Kidderminster,*
 “ *in his Dissertation on the Malignant Epidemi-*
 “ *cal Fever of 1756.*”—He then quotes the pas-
 sage at p. 51 of my father’s Dissertation. “ The
 “ spirit of sea salt has been a *common* ingre-
 “ dient in the steams made use of by my fa-
 “ ther for the ulcerated sore throat ; this is
 “ now become the *usual practice in this country,*
 “ and every day produces fresh instances of its
 “ salutary effects. The following is the most
 “ convenient method : Let a quantity of com-
 “ mon salt be put into a stone or earthen jar,
 “ and

“ and let the jar be placed in a vessel contain-
 “ ing warm water or heated sand; by these
 “ means a constant gentle heat may be kept
 “ up, and a very gentle heat is sufficient to
 “ raise the marine acid in its volatile aerial
 “ state, by pouring oil of vitriol on the salt;
 “ after this more of the vitriolic acid may be
 “ added as occasion requires. If the vessel be
 “ placed at a considerable distance from the
 “ bed side, the patient’s lungs will not be in
 “ the least irritated; nay, it is possible it may
 “ be raised in so gentle a degree as even to be
 “ serviceable in cases of ulceration of the lungs;
 “ but this still remains to be tried. It is cer-
 “ tain that where it has been tried no incon-
 “ venience has been observed to arise either to
 “ the patients or attendants. *Experience has*
 “ *already shewn* the signal utility of this acid
 “ air in putrid fevers, and malignant fore
 “ throats, and joins with theory in recom-
 “ mending it, not only as a purifier and restorer

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“ of

“ of infected air, but as a strong resister of putrefaction, and a most *eligible form* of administering a powerful and efficacious remedy against all putrid disorders.” The paper from which the above sentences are extracted was sent by my father to Dr. Priestley. The following is the doctor’s answer :

Calne, 10th July, 1775.

SIR,

I should have acknowledged the receipt of your favour, inclosing your son’s ingenious remarks on the medical virtues of the different kinds of air ; but having lately removed from one house to another, I have been obliged to neglect most of my correspondents.

As your son’s paper contains only opinions and proposals, the Royal Society would not receive it, as they make it a rule to admit nothing that is not supported by new facts. They attend to nothing else, be it ever so ingenious
and

and probable. Now I could wish, as your son seems to have a genius turned that way, he would make experiments in order to ascertain the favourable or unfavourable effects of different kinds of air on the human body. It is a very extensive field of inquiry, and could not fail to reward his application. In one respect your son has mistaken my meaning, for *I did not find that acid air, corrected air injured by putrefaction, but the contrary.* It may, however, have a salutary effect on the air, in various states. If he can ascertain that, it will be a curious and valuable discovery.

I have been pretty fortunate in the prosecution of my experiments since the publication of my last volume, and I have now another in the press, containing a great variety of new facts, and some of them I think of considerable importance. Among other things I have found that by means of nitrous acid and earth I can make air either of the same degree of

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purity

purity with the air of the atmosphere, or air five times as good. This, I should think, is capable of very important medical uses, but those things I leave to physicians.

I should be much obliged to you for a sight of the Dissertation you mention as having been presented to the Royal Society.

With my respectful compliments to your son,

I am, Sir,

Your very humble servant,

(Signed)

J. PRIESTLEY.

(Directed)

Dr. Johnstone, Kidderminster.

My brother, it should be observed, writes in the foregoing paper as a physician, and Dr. Priestley as a chemist. My brother mentions not only opinions but facts; he does not describe them circumstantially and in detail, but he refers to them explicitly and generally; and those facts include cases in which my father
had

had used the marine acid effectually for destroying contagion. I cannot account for my brother's misconception of Dr. Priestley's opinion, and I need not apologise for it, because my subject requires me to state what my father practised, and what my brother knew in the cure of putrid disorders, by the marine acid.

Surely I have produced documents that prove my brother James to have written a Treatise on the medical virtues of different kinds of air, in 1775 ; in which he particularly notices muriatic acid gas as the method recommended by his father in 1756 to correct vitiated air, as a purifier and restorer of vitiated air, and a strong resister of putrefaction ; an agent capable of a very important medical application, *not mentioned by Dr. Priestley*, and his medical correspondents ; an efficacious and powerful remedy against all putrid disorders, and for the application of which he gives precise directions ; a remedy which, if administered

sidered with caution, will not prove inconvenient either to the patients or attendants. Will Dr. Smyth say now that *no idea* of the power of mineral acid gases existed in the mind of my brother before 1779? Will he say now that my brother owed his knowledge of acid air to Dr. Priestley? Will he venture to assert now that Dr. Priestley owed his knowledge of it to Guyton-Morveau? Dr. Priestley declares, that *he did not find acid air, corrected air injured by putrefaction*. My father, in 1756, had found that it *did*, and M. Guyton-Morveau in 1773 had found the same. What then becomes of Dr. Smyth's position? "And if his son, Dr. James Johnstone, shall be thought to have made any approach towards it, *he owed this entirely to Priestley*, and he probably to Morveau." According to Dr. Smyth my brother borrows from Priestley an opinion which the supposed lender disclaims, and Priestley filches from Morveau, an opinion

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nion which Morveau never held. Thus stands the order of facts. Acid air *has* a certain effect in the judgment of my brother and Morveau. Acid air has *not* that effect according to Priestley. But, according to Dr. Smyth's account, my brother borrows from Priestley an opinion which Priestley, the supposed lender, never held; and Priestley filches from Morveau an opinion which he never held. It does *not* appear that Priestley had ever seen Morveau's account, but it *does* appear that my brother had seen Priestley's account of acid air, and had drawn from it an inference which the doctor does not admit. And it also appears that my brother derived his first knowledge of the acid air from my father. All, therefore, that my brother owed to Dr. Priestley was the term acid air; a term certainly not more correct than the term marine acid, which he had previously learned from his father's Dissertation on Fevers. The whole transaction proves
that

that my father was anxious to inform Dr. Priestley of his own discovery. My brother's paper is chiefly occupied with this part of the subject, and it appears to have been the chief aim to have made it public through Dr. Priestley. The Doctor, however, in his letter takes no notice of my father's having so long before used muriatic acid gas, but desires to see the Dissertation. Dr. Priestley's letter is precious to all men of science, as giving a very early intimation of the discovery of oxygen gas. It is important to medical men, as containing a proof that information had been communicated by my brother to Dr. Priestley in 1775 of the application of muriatic acid gas to correct air injured by putrefaction. It is important as a corrector of Dr. Smyth's assumptions, even in the very instance where my brother misunderstands Dr. Priestley, and Dr. Priestley dissents from my brother; for the Doctor plainly says that, according to his experience,

“ acid

“ acid air did *not* correct air injured by putrefaction ;” and my brother says, according to his experience and his father’s, the acid air *did* correct air so injured.

Thus it is clear that my brother did not receive the hint from Priestley, and that Morveau did not give the hint to Priestley, (for Priestley never received it till he got it from my brother). Whether Dr. Smyth, or any other person, received it, without acknowledging it, from my father, or my brother, or Morveau, I know not.

My mention of the manuscripts of my father, as not affording any precise evidence, Dr. C. Smyth has converted into circumstances supporting the claim the first in order as in importance. His own expressions are, “ the first of which, in order as in importance, is his father’s manuscripts ;” alluding to what Dr. Smyth calls the other circumstances, which I have stated, to support my

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claims.

claims. So far from stating *these* manuscripts as the first in importance, I declared that it would be useless to enter upon them, because they consisted merely of names and prescriptions. That I may be completely understood, however, I shall insert one or two cases, in the manner in which my father posts them down, as an illustration of my meaning:

1. Mrs. Bennet—*Angina die quarto—Eruptiones die quarto—Gargarisma. Vapor Acid: Spir: Sal: Marin.—Sp. Nitri dulc: ʒi g^{ss} instilland: potu, ad gratam aciditatem.*

2. Mr. Winton, *æt: 18. febris putrida cum Delirio, Petechiis, Exanthemat: die 10.*

3. Mrs. P. *æt: 45—Tussis, febris nervosa putrida, filio mortuo infecta, die 8.*

Pro Winton, Vinum liberrime, successio Vescantium, cortex, et antiseptica mineralia liberrime.

The notes of physicians are seldom more full than these; and it is only in connection with
other

other proofs, that such notes can at all be considered as satisfactory documents. Alone they would at best afford only presumptive evidence; and, resting my father's cause upon the more direct evidence employed in my pamphlet, I thought it unnecessary to adduce them in my statement. They are not circumstances the first in importance, according to my statement. But they are the next in order after the testimony supplied by my father's book, and the next in order before the mention of the testimony of Mr. Crane, an accomplished professional man, who knew my father's practice, and was daily concerned in it, thirty years ago, and who is prepared to give the fullest testimony, "that the muriatic acid vapour was so
 "commonly used when he first settled in business, that the manufacturers placed it spontaneously in their shops, when fever was apprehended, and that in malignant cases it
 "was always ordered by my father." But

about that testimony which is next in order to my father's cases, as they are stated in my father's manuscripts shortly and imperfectly, and far superior in importance, Dr. Smyth preserves a deliberate and mysterious silence ; whilst he rests with double attention upon that very part where he ought to have shewn that it was futile, or admitted that it was pertinent and conclusive, which I do not adduce as evidence, and declare I think it would be unnecessary to adduce. He does indeed, in another part of his letter (p. 23) mention that testimony ; and the mention of it here is accompanied by circumstances equally remarkable with the omission of it elsewhere. No opinion is given upon its value, no comment is made upon its contents, no resistance is attempted against its force. From secret contempt, or secret fear, from blameable negligence or even blameable artifice, Dr. Smyth is content with saying, that the only proof which

I have

I have brought of my father's using the marine acid, excepting my own assertion, is the testimony of a surgeon at Kidderminster. This position is in two respects inaccurate.* I have brought many *other* proofs, or at least what seemed to me such, *besides* the testimony of Mr. Crane and my own assertion. I spoke of Mr. Crane not merely as a *surgeon*, but as an eminent and respectable surgeon in Kidderminster; and his claim to such appellations is not weakened, because Dr. Smyth may have never heard of his name till he saw my pamphlet; just as he professes, p. 31, never to have heard of my father's name, till my anonymous para-

* Quid iniquius quam ubi ex altera parte, longe probabiles et firmissimæ rationes existant, ex alterâ infirmæ admodum; tamen ideo rem ipsam venditare pro dubiâ incertâque, et inter rationes illas paritatem et ἰσοσθένειαν admittere, quamlibet ἀδοξασως, quod nihil est, quam ludere in re maxime seriâ, et ἀδοξαστως insanire.

FABRICIUS, in Præfat. Sext. Emp.

graph

graph in the Morning Chronicle led him to enquire for my father's book.

But to return to my father's manuscripts. Though my father's manuscripts, so far as they were known to me, had consisted merely of names and prescriptions, without any regular statement of symptoms, or of the particular operation of medicines, I contend that they contain such a statement as proves muriatic acid gas to have been used by my father subsequently to the year 1756, and, I now add, previously to the appearance of Dr. Smyth's book in 1795, and to his possible practice at Winchester in 1780. The same statement, though not regular, is clear enough, and full enough to confute Dr. Smyth's inference from my pamphlet, "that, upon the authority of my own representation, my father's manuscripts do not furnish a single example of the successful employment of the marine acid, *any* opinion of its safety, *any* evidence
" or

“or even *suspicion* of its use.” I have a volume of my father’s manuscripts now before me, and from that volume I will produce passages which demonstrate, in several examples, the successful employment of muriatic acid gas; which shew my father’s just and fixed opinion of its safety, which contain evidence not merely for his suspicion, but for his distinct and thorough knowledge of its use.

The following is an extract from my father’s journal, dated Kidderminster, 10th March, 1776. “I have attended several patients ill
“of a low putrid fever, and even in some a
“petechial fever.” It was at this time and with this fever that my brother was attacked; and in whose cure I remember muriatic acid gas to have been employed, as appears by the following:

“My dear son Edward had a very copious hæmorrhagy from his nostrils on the 7th day
of

“ of this fever. He had been infected by
 “ too closely observing some patients ill of it.”
 At the close of his directions for the cure of
 this fever, my father says, “ I always enjoin
 “ washing the patient with vinegar and cold
 “ water, to have his linen often changed ; if in
 “ a state of perspiration, his feet are wrapped
 “ in flannel, wet in warm vinegar ; and to *fill*
 “ *the air with an antiseptic vapour, I order*
 “ *oleum vitrioli, to be poured on sea salt, by which*
 “ *its acid fume is diffused* in the chamber. I
 “ also order vinegar to be sprinkled or boiled
 “ in it,” (*i. e.* the chamber).

10 Dec. 1800. “ These observations, sent
 “ in a letter to Dr. Lettsom for the Medical
 “ Society, are thus introduced ;

“ DEAR DOCTOR,

“ I send you the following notes as a friend-
 “ ly endeavour to divert your grief and cares,*

* The son of this excellent man was recently dead.

“ and

“ and as a small contribution to the under-
 “ taking of the Medical Society, and mark of
 “ attention to them. These observations re-
 “ late to a fever which prevailed in some parts
 “ of this country and in Kidderminster in
 “ 1775, 1776. In treating this and some
 “ other fevers which had *before* occurred, I
 “ adopted practical measures, which appear
 “ to have been successfully tried by other *later*
 “ practitioners. It is not so much my view
 “ to claim priority in practice, as to confirm
 “ thereby attempts to cure by similar means,
 “ and to obtain additional credit to my ob-
 “ servations, from the experience of other
 “ able physicians, Carmichael Smyth, Currie,
 “ &c.”

In another manuscript on a general view of
 fevers and of their cures, in which my father
 gives the result of his long experience, or ra-
 ther has left it a legacy to his family and poste-

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rity,

rity, the subject of fumigation is thus treated. The manuscript was probably finished the 6th Dec. 1799, as this date is inserted at the close.

‘ The vapours which arise from common
 ‘ salt and from nitre are powerful correctors
 ‘ of air; the vapour of the last restores the
 ‘ consumed oxygen and vital air, the former
 ‘ adds to its antiseptic power, and resists pu-
 ‘ trefaction. *With this view it was recom-*
 ‘ *mended by me ever since the year 1756; and*
 ‘ is mentioned in the Dissertation on Fevers
 ‘ which have prevailed at Kidderminster; I
 ‘ have often had occasion to apply it from that
 ‘ time. It is easily procured by pouring a
 ‘ little ol. vitrioli on dried sea salt. It rises
 ‘ by a very moderate degree of heat, and dif-
 ‘ fuses itself very widely in every part of the
 ‘ habitation of the sick, and if not too near
 ‘ them, without the least inconvenience to
 ‘ them

‘ them or their attendants. It refreshes those
 ‘ that are well along with the sick, and pre-
 ‘ vents the danger of infection.”

‘ It appears by a memoir presented, I believe,
 ‘ about the year —— by M. Lavoisier, and pub-
 ‘ lished in Memoirs of the Royal Academy of
 ‘ Sciences, that the celebrated M. de Morveau
 ‘ made use of this method to purify the air in
 ‘ infected prisons. The method, as described
 ‘ by him, is perfectly similar to that recom-
 ‘ mended by me *long before*, “ and consists in
 “ disengaging and diffusing a great quantity of
 “ marine acid, in a state of vapour, in the in-
 “ fected places. For this purpose a half of a
 “ quarter measure of marine salt, more or less
 “ according to the size of the chamber, must
 “ be heated in a large iron spoon, or a small
 “ pan ; when the salt is well heated a quan-
 “ tity of oil of vitriol, amounting to one-third
 “ or half the weight of the salt, is to be poured

“ on it in the same vessel ; after which every
 “ one retires, and leaves the door of the room
 “ shut. The vitriolic acid by its action on the
 “ marine salt disengages its acid ; and the lat-
 “ ter rises in the form of a white vapour, which
 “ diffuses itself through the whole chamber,
 “ and neutralises the putrid particles, by which
 “ it was infected.’ Dr. James Carmichael
 ‘ Smyth, in an ingenious and important work,
 ‘ describing his treatment of the gaol distemper,
 ‘ has improved the method of destroying con-
 ‘ tagion, which he believes to be of a putrid na-
 ‘ ture, by employing the acid vapour of nitre,
 ‘ raised also by means of ol. vitrioli, for that pur-
 ‘ pose. He has proved, in unexceptionable ex-
 ‘ periments, that it may be safely breathed by
 ‘ the sick, and by those that are well. And in
 ‘ private and in hospital practice, says he, p. 194,
 “ I can declare with truth, that when the ni-
 “ trous acid has been used constantly as a fu-
 “ migation,

“ migration, I have not known an instance of a
 “ contagious fever having been communicated
 “ even to a nurse or an attendant.”

‘ The Doctor, p. 193, with a due zeal, claiming priority of invention, says : ‘ From all the
 “ information I can procure, I do not find that
 “ any person has ever made use of the nitrous
 “ acid to destroy contagion but myself.’
 ‘ When nitre cannot be had, the Doctor has no
 ‘ hesitation in employing the marine acid, ‘ as
 “ in a moderate quantity it may be used safely,
 “ where people are present.’ p. 191.

‘ I hope I also may be excused in presenting
 ‘ to posterity my claim to an early use of marine
 ‘ acid to destroy contagion in putrid fevers.
 ‘ This method of correcting contagion is suggested in my Historical Dissertation on the
 ‘ Putrid and Malignant Diseases which reigned
 ‘ at Kidderminster; London printed, 1758.
 ‘ Also, in an Essay on the Ulcerous Sore
 ‘ Throat, by my son deceased.

‘ I add

' I add here, that ever since that time it has
 ' been constantly directed by me to be used in
 ' work-houses, gaols, and in private practice,
 ' with a success which has recommended it in
 ' this country. I do not mean to deprive M. de
 ' Morveau, and the French chymists and phy-
 ' sicians, nor Dr. C. Smyth, of the honour due
 ' to them, for applying and extending this in-
 ' vention, especially for more public and exten-
 ' sive use. But I also have a claim to assert in-
 ' vention; and perhaps the spark of light struck
 ' out by me may have kindled a more splendid
 ' blaze. What I recommended in the obscurity
 ' of my retired situation may gradually have
 ' made its way to the attention and experience
 ' of these learned men, aided by a more exten-
 ' sive and correct knowledge of chemistry.
 ' NEVERTHELESS, I ALSO AM AN INVEN-
 ' TOR.'

Such is the candid and respectful language
 concerning Dr. Smyth, deliberately and fe-
 riously

riously employed by my father, employed by
 that very man, whom that very Dr. Smyth re-
 presents as ‘unacquainted with any distinction
 ‘in the application of the sulphureous and ma-
 ‘rine acids; as considering them as equally safe
 ‘and equally useful; as giving common direc-
 ‘tions (and he must mean only common)
 ‘how they are procured, and cautions respect-
 ‘ing their use, too inaccurate to be of much
 ‘service; as making under his own hand a de-
 ‘claration in my brother’s thesis, which sets
 ‘aside all pretensions to the discovery claimed
 ‘in his name; as having put to paper writing
 ‘which furnished not a single example of the
 ‘successful employment of the marine acid, no
 ‘opinion of its safety, no evidence or even
 ‘suspicion of its use; as unknown to him (*i. e.*
 ‘Dr. Smyth,) even by name, until his son’s
 ‘anonymous paragraph in the Morning Chro-
 ‘nicle had led Dr. Smyth to enquire for his
 ‘book of which, with some difficulty, he
 ‘procured

‘procured a copy, and from which, if he had
 ‘been possessed of it at an earlier period, *he*
 ‘*could have derived no useful information.*’

My indignation kindles while I transcribe these contemptuous passages, but I ought to produce them, because they will assist the reader in contrasting Dr. Smyth’s rudeness and injustice to my father, with my father’s prompt justice and unfeigned respect to Dr. Smyth.

After reading the foregoing passages from my father’s manuscripts, is Dr. Smyth so stubborn as to retain, or so hardy as to avow his doubts, whether those manuscripts afford proof of muriatic acid having been used by him subsequent to the year 1756, and I will add before 1780? Will he insult the candour or the sagacity of professional men by maintaining, that the report which I now make of my father’s manuscripts will not much add to his character in their opinion? Will he venture to tell me again that “my zeal is intemperate,
 “ and

“ and injures the person whom it meant to
 “ serve ?” Will he persist in maintaining
 that my brother’s inaugural thesis “ is to be
 “ looked upon as a declaration under Dr. John-
 “ stone’s own hand, which at once sets aside
 “ all pretensions to the discovery claimed in his
 “ name ?” If my brother’s silence in the thesis
 published in 1773 were an hypothesis to be
 admitted as a proof, that neither he nor my
 father had at that time the least knowledge
 of the mineral acids, nor had ever employed
 them in practice ; still my father’s explicit
 declaration, *really* made in his own hand-wri-
 ting, must be allowed to prove that in 1776
 he had somewhat more than a *little* knowledge
 of the powers of the mineral acids, and had in
more than one instance employed them in prac-
 tice. Thus, what my father actually did
 record in his own hand-writing, supports that
 very pretension to discovery, which the decla-
 ration he is *falsely* supposed to have made in his

own hand-writing, is pronounced by Dr. Smyth to have at once set aside. When Dr. Smyth, from conjecture, confidently imputes to my father what he did *not* do in my brother's thesis, and, by *inference*, as confidently denies what my father *had* done in his own manuscripts, I must take the liberty of telling him, that *his* zeal in refuting me, is not very creditable to his caution or his candour, and that it may ultimately injure the person whom it meant to serve.

Well—Dr. Smyth may say, that whatever may have been my father's practice in 1776 there is no evidence for the uniformity of his employment of muriatic acid gas after 1758; and indeed my alert and sagacious antagonist has explicitly said, from my brother's thesis in 1773, “that neither father
“nor son had in that year the least knowledge
“of the power of the mineral acids, or had
“ever employed them in practice,” p. 17. The acuteness of Dr. Smyth's logic, when he
is

is criticising my brother's thesis, is accompanied by a lamentable want of memory as to the contents of my pamphlet; and, while he with excess of eagerness pursues his chance of victory in one quarter, he inadvertently exposes himself to attack from other quarters. Now unless Mr. Crane, a *disinterested* person, lied to me, or Mr. Cooper and Mr. Symonds, two persons equally disinterested, told lies to Mr. Crane, my father before 1773 *did well know* the power of the mineral acids, and *had often* employed them in practice. Nay more, unless my brother told a lie in 1779, my father had long ago *ordered* the marine acid for the correction of vitiated air; and if a physician orders a medicine for his patient, even though he should not recommend it to other professional men, the medicine so ordered, involves a proof of his knowledge, and constitutes a part of his practice. The knowledge may be small, and the practice may be wrong. But I seriously

hope not to incur the displeasure of Dr. Smyth, as a grammarian, or as a dialectician, when I say, that *long-ago* is not explained in our English dictionaries by the word *lately*; that *sometimes* is not the usual synonyme for *never*; that *any* degree of knowledge differs from *not the least* knowledge; and that *any* degree of practice is not quite the same thing with *no* practice at all.

Perhaps the experiments with mineral acid gases, made by Dr. Smyth at Winchester induced him to believe, that in point of practical utility, a very little knowledge and a very little success approximate to total ignorance and total failure. In such a case the good sense of Dr. Smyth might be permitted to check the impetuosity of his logic, and suggest to him, that things so near to each other in reality, hardly deserved to be distinguished in terms.

Dr. Smyth next proceeds to a Treatise on Malignant Angina, which *was* the work of
deliberate

deliberate reflection of my brother James; whereas his thesis was merely the necessary exercise for his degree; a publication, says Dr. Smyth, with which I have taken the same liberty as with my father's, "altering the language, making particular quotations, and suppressing entirely what does not suit his purpose." How utterly unjust and unfounded this assertion was, when applied to the quotation I made from my father's book, is already shewn. The alteration in language which escaped me in the quotation from my brother's book is the word "recommended," instead of the word "ordered," and this is all. To bluster about such an alteration, under any circumstances, is surely very unworthy of the discussions of gentlemen and men of science. In the present instance it is clearly a mistake; the term "recommended" is of weaker import than "ordered;" and to cavil about it shews the

extreme

extreme distress, the last agony of the advocate for a weak and expiring cause.

In the quotation from my brother's book * I brought forward the passage which proved that both father and son had a knowledge of muriatic acid gas, and that both did employ it to correct contagion. I did not think it necessary to cram my pamphlet full of quotations. I did not even cite all the passages that proved the fact. It is, however, neither my wish to alter the language, nor to make partial quotations, nor to suppress that which does not suit my purpose. I shall rejoice if men of science will take the trouble of reading both the books from which I have quoted, and I will consent to be called an idiot if, after an impartial perusal, they do not decide in favour of my father's claim.

* Treatise on Malignant Angina, &c. by James Johnstone, M.D. Physician at Worcester; Worcester printed, 1779.

That

That I did not quote all the passages that suited my purpose shall now appear—I did not quote the following:

“ The ulcerated fore throat had for some
 “ years past very rarely appeared; in those few
 “ instances which did occur its malignity was
 “ much abated, and the management of it be-
 “ came so familiar in many places that the
 “ good women themselves cured it by means
 “ of the bark and antiseptic steams.” Treatise,
 p. 4.

“ Antiseptic steams composed of vinegar,
 “ myrrh, and honey, or the acid air raised by
 “ pouring oil of vitriol upon sea salt; gargles of
 “ vinegar, or tincture of roses with spirit of
 “ salt; blisters to the throat or other parts of
 “ the body; and mixtures, with a large quan-
 “ tity of the bark rendered gently diaphoretic,
 “ a generous antiseptic diet, and vinous sub-
 “ acid drinks, constitute the only successful
 “ method

“ method of cure. This method, applied early
 “ and continued with due perseverance has
 “ very seldom failed,” p. 6.

“ The patient’s chamber filled with these
 “ vapours (acid air) is the surest method of
 “ preventing contagion,” p. 111.

These sentences I did not quote, though they suited my purpose; but if Dr. Smyth read them, he probably found that to produce them could not suit *his* purpose. I shall now produce the whole of another passage, part of which may be found in my pamphlet, and has been the subject of animadversion from Dr. Smyth.

“ Antiseptic vapours (says my brother*)
 “ are also to be considered as remedies of the
 “ first importance. The steams of myrrh and
 “ camphor boiled in vinegar and honey are of
 “ great use in cases of angina maligna, by fa-

* Page 109.

“ cilitating

“ cilitating respiration and increasing the flow
 “ of saliva. It has already appeared (ch. 3. § 4,)
 “ that the air thrown out from the lungs
 “ is highly charged with putrid effluvia, and
 “ that these effluvia occasion the parts about
 “ the throat, fauces, and nostrils, to be parti-
 “ cularly affected with malignant ulcers. The
 “ antiseptic vapours above mentioned are most
 “ immediately and essentially serviceable in
 “ correcting any putrid quality of the air,
 “ when it is either drawn into or breathed out
 “ of their lungs; by this means the *fomes* of
 “ the disease is altered and counteracted, at the
 “ same time that the parts already ulcerated
 “ are cleansed and healed. The humid vapour
 “ of vinegar, with honey, or myrrh, or cam-
 “ phor, should be drawn into the mouths of
 “ the sick, as often as they can conveniently
 “ do it. For this purpose there are *steaming*
 “ *pots* fitted with proper pipes to convey the
 P “ vapour

“ vapour with greater force into the lungs,
 “ which, being generally known, need not be
 “ described here. It is no inconsiderable ad-
 “ vantage of such vapours that they encourage
 “ a discharge of putrid lymph from the glands,
 “ while they act as topical correctors of putre-
 “ faction throughout the whole extent of the
 “ fauces, trachea, and ramifications of the
 “ bronchia. As it is impossible too cautiously
 “ to guard against the effects of so putrid a
 “ contagion, the acid air or spirit of salt should
 “ be kept rising continually in the room, by
 “ pouring oil of vitriol once or twice a day on
 “ sea salt, placed in a convenient vessel. This
 “ spirit will rise in the moderate degrees of heat
 “ from 60° to 70° of Fahrenheit, so as to be
 “ perceived in every part of the room by its
 “ penetrating acid smell. *This method of cor-*
 “ *recting vitiated air, which is useful in this*
 “ *and every other putrid disease, was long ago*
 “ *ordered*

“ordered by my father, and is now recommended by Dr. Priestley. To have the patient's chamber filled with these vapours is the surest method of preventing the spread of contagion.”*

So wrote my brother in 1779, about the power of muriatic acid gas; and if language can convey in precise terms an obvious meaning it is done here. At first he recommends vinegar, &c. as topical correctors, “by facilitating respiration and increasing the flow of saliva,” and for cleansing and healing the parts already ulcerated, by correcting any putrid quality of the air. For these purposes there are steaming pots; and, to sum up their whole power as topical applications, he adds, “it is no inconsiderable advantage of such vapours that they encourage a discharge of putrid lymph from the glands, while they

* Johnstone's Historical Dissertation concerning the Malignant Fever of 1756, p. 51.

“act as topical correctors of putrefaction
 “throughout the whole extent of the fauces,
 “trachea, and ramifications of the bronchia.”
 But “as it is impossible too cautiously to
 “guard against the effects of so putrid a con-
 “tagion, the acid air should be kept rising
 “continually in the room,” &c. The next
 paragraph after this I did not quote in my
 pamphlet, and it is not noticed by Dr. Smyth,
 “to have the patient’s chamber filled with
 “these vapours is the surest method of pre-
 “venting the spread of contagion.” Did Dr.
 Smyth read *these* words? Understand them
 he must; notice them he did not. But, with
 a contemptuous disregard of their real and
 plain import, he maintains that my brother
 was evidently without experience; though
 upon that subject he gives the most exact and
 salutary directions that could flow from the
 pen of the most experienced practitioner: that
 my brother was ignorant of the powers of the
marine

marine acid gas, though that very brother speaks of that very acid gas as the surest method of preventing the spread of contagion. My brother did know that power; he communicated it to Dr. Priestley in 1775, he communicated it to the world in 1779. And yet it suits the logic of Dr. Smyth to believe, or his morality to affirm, either that he published what he did not know at all, or that his knowledge in 1779 was not prior to Dr. Smyth's knowledge, gained by Dr. Smyth's experience, in 1780. He either then published what he did not know, or his knowledge possessed and declared in the paper of 1775, published in the book of 1779, was not prior to Dr. Smyth's knowledge, gained perhaps in part at Winchester in 1780, and published to the world in 1795. Thus it appears, surely, that my brother does recommend the use of muriatic acid gas to prevent the spread of contagion, and that he knew his father to have used it;

and

and that he himself had experienced its use is clear, from the mention he makes of its sensible effects, "that its penetrating acid smell
" may be perceived in every part of the room."

As to the inconvenience which an imprudent use of it might occasion, we may presume that he had read the very book of his father, which he quotes; that he understood it, that he remembered it, and therefore would not slight the caution which his father had suggested for avoiding that inconvenience. He says expressly, that his father had ordered it *long ago* for correcting vitiated air; and when he himself calls it a method of correcting vitiated air, which is useful in every putrid disease, surely he recommends it. Nay, he again recommends it, when in the very next paragraph he again tells us, "to have the patient's chamber
" filled with these vapours is the surest method
" of preventing the spread of contagion."
Will Dr. Smyth then persevere in contending
for

for the distinction which he sets up between what my father ordered and recommended? Will he suppose that my father, in frequent and confidential conversations with my brother, did not *mention* the success of that which he ordered; and of this success can any expressions be more declaratory than those used by my brother?

I hope Dr. Smyth will be convinced, before the termination of this reply, that *my* knowledge of my own language does just enable me to make some distinction in the meaning of the words *ordered* and *recommended*; and I trust that I shall have just sense enough to impress my knowledge of the meaning and of the value of words in such a way, as shall deserve to be remembered by Dr. Smyth, though, perhaps, the Doctor may be disinclined to mention it.

Whether in his treatise my brother does not recommend muriatic acid gas as a remedy of more efficacy than vinegar, in my opinion,

opinion, cannot be a matter of doubt. The antiseptic steams of vinegar, &c. are his topical correctors; they are a species of gargle to be drawn into the mouth from steaming pots, and which will correct the putrid matters within the fauces. But by muriatic acid gas he guards against effects; *they* are useful in that and all other putrid disorders; *they* are the *surest method* of preventing the spread of contagion. Dr. Smyth says, notwithstanding, “marine acid gas is recommended by him, “not as a remedy of more efficacy than the “steams of vinegar, not as the sole preservative “against contagion, but as an additional security;* whereas Dr. Johnstone, by omit-

* Dr. Smyth here acknowledges that in 1779, as “an additional security” against contagion, the acid air is recommended by my brother. If my brother knew in 1779 that mineral acid gas afforded any security against contagion, then could not Dr. Smyth *discover* the same principle of security in 1780. In Dr. Smyth’s memorial presented to Lord North, his services at Winchester are enumerated at full length as the grounds of
his

“ting what his brother says of medicated vinegar, wishes to have it believed that the

his claim to remuneration ; but not a syllable is said about discovery. On the contrary, he alludes to purification as his chief agent, “and that these measures, if fully carried into execution, “would have entirely destroyed the contagion is extremely “probable ; as none of those Spaniards who were washed in the “river, new clothed, and lodged in the purified prison wards, “were seized with the distemper ;” Smyth, p. 229. If he had then known, or suspected he had known, a method of purifying them in the hospitals, would he not have employed it ? In another part of the memorial he says, “nor was it possible, however necessary, without compulsion, to take from the Spaniards “all their old clothes and bedding, which are known so powerfully to retain the seeds of contagion.” But the deliberate reflection of Dr. Smyth had before instructed him “that nothing “could so certainly or efficaciously destroy it (putrid contagion) “as mineral acids in a state of vapour ; but how to employ these “with safety was the difficulty : *hoc opus, hic labor.*”

Now surely there could be neither danger nor difficulty in exposing *the old clothes* to this certain and efficacious power in any of its shapes !

Periculosæ plenum Opus alex
Tractas, et incedis per ignes
Suppositos cineri doloso.

Q

“ acid

“acid air was only employed.” I had no such wish; I must have been a fool to have had such a wish. Vinegar in private practice is an useful antiseptic, and so useful as to justify my brother in saying that it was immediately and essentially serviceable for the purposes subjoined. It could not be omitted in a treatise on malignant sore throat: my brother, therefore, properly notices it as a topical corrector, and then enforces the use of muriatic acid gas as an useful agent for correcting vitiated air in putrid diseases, and the surest method of preventing the spread of contagion.

In my pamphlet I wanted to establish the fact that my father knew and applied the powers of muriatic acid gas in destroying contagion; one of the proofs was the assertion of my brother that this acid gas had been ordered for this purpose long ago by my father. The quotation I made was quite sufficient for my purpose, and therefore I did not quote the preceding

preceding nor the succeeding paragraphs. I was guilty then of deliberate omission, and I have assigned my reasons for it. But the same omission in Dr. Smyth deserves the name of suppression. The Doctor maintains here that in the opinion of my brother the muriatic acid gas was *not* a remedy of more efficacy than the steams of vinegar ; *not* the sole preservative against contagion, but “ an additional security.” Yet Dr. Smyth, when he read the next paragraph, must have perceived that in my brother’s opinion it *was* of more efficacy than the steams of vinegar in preventing the spread of contagion ; that it was a *better* preservative against contagion ; that it was the *strongest* security ; that it was even a method the *most* sure.

The main point in dispute between Dr. Smyth and myself, as I must again and again repeat, is priority in the discovery, and in the application of the mineral acid gases to destroy

contagion. I omit in my brother's evidence what is unnecessary to my purpose of shewing that the principle and application could be claimed for my father long before they could be claimed for Dr. Smyth. Dr. Smyth suppresses the same evidence, for the purpose of shewing that my brother was not acquainted with the comparative efficacy of muriatic acid in cases of contagion. But further, if what my brother says in the first paragraph had implied no more than Dr. Smyth erroneously assigns to it; if it had represented the marine acid as a security against contagion inferior to vinegar; still it proves that the marine acid was known to my brother and my father before 1780; that it was used by both of them before that time, and that longer experience was then necessary to unfold to them the comparative value of the principle. But that experience in point of fact they really had, and having it, my brother says, "it is the surest
 "method,"

“ method,” &c. If then in the year 1779 my brother knew, as Dr. Smyth concedes him to have known, the muriatic acid gas to be an additional security against contagion ; if he further pronounced that additional security, as Dr. Smyth knew, and ought to have remembered that he *did* pronounce it, to be the surest method of preventing the spread of contagion; it contains two circumstances most pertinent to my argument, and most favourable to my cause. It contains, first, a proof that the principle was positively known by experience ; and, secondly, a proof that the extent of the principle also was known. It establishes what Dr. Smyth would disprove, the real priority of the discovery. It confutes all the reasoning adduced by Dr. Smyth against that reality, from the limited advances of the discoverer in theory, and the limited use of the preventive itself in practice.

I shall trust to the candour of all who can
read,

read, and who reading can understand, whether it be possible to infer now, “ that Dr. James Johnstone was led solely to the mention of the marine acid from the recommendation of Priestley.” The reason why he mentions Dr. Priestley’s name (and it is a name venerable and memorable in the annals of science) is clearly, because in the paper before quoted, he had communicated with him in the year 1775 on this subject; in which year Dr. Priestley certainly had not considered, if he at all knew, Guyton de Morveau’s experiments.

At p. 22 of the Letter to Mr. Wilberforce Dr. J. C. Smyth enters upon the topic of my ignorance. He introduces it in the following words: “ He (Dr. Priestley) therefore must have seen Morveau’s Memoir, published in the year 1773, first in the Memoirs of the Academy of Dijon, afterwards in the Journal de Physique of the same year, although Dr. John

“ John Johnstone, with his usual accuracy, af-
 “ firms that no account of Morveau’s experi-
 “ ments was published until ten or twelve
 “ years after the year 1773, and then in the
 “ Memoirs of the Academy of Sciences. This
 “ gentleman’s ignorance on a subject where
 “ he conceives his father’s honour so imme-
 “ diately implicated is scarcely to be credited,
 “ but I leave the explanation to himself.”

In the foregoing passage I am confidently ac-
 cused of general inaccuracy; I am sarcastically
 described as “ this gentleman ;” I am point-
 edly charged with ignorance in the vindication
 of my father’s character scarcely to be credited;
 I am contemptuously left to explain what, in
 the judgment of my accuser, admitted no ex-
 planation consistent with my credit as a son,
 a physician, and an observer of truth.

Upon this occasion I must say,

Μαροῖς μωρίαν ὀφλισκανω.

SOPHOCLE in Antig.

When

When I alluded to the period of the publication of Morveau's experiments at Dijon, I explicitly stated in a note that I quoted from memory. I had not seen Morveau's *Traité des Moyens de désinfecter l'Air*, when I wrote my pamphlet; and I did not then know that his method of fumigation had ever been noticed in the *Journal de Physique*. By M. de Morveau's own account, the Memoir on the Fumigation of the Cathedral was not first published in the *Memoirs of the Academy at Dijon*; it was the account of a second attempt to purify the jail at Dijon, of which "*M. Maret, secrétaire perpétuel de la ci-devant Académie de Dijon, fit insérer la notice dans le Journal de Physique de Janvier 1774.*"* The account of Morveau's experiments was afterwards published in *Mem. de l'Acad. R. des Sc.* 1780. Such was the source of what Dr. Smyth calls *this gentleman's ignorance*. I had

* *Traité des Moyens*, p. 12.

seen the account published in these Memoirs ; I did not know that it had ever been published in the Journal de Physique ; I had not the book at hand to quote, and therefore openly declared that I quoted from memory lest there should be mistake. Was it worthy of Dr. Smyth, was it generous in him, was it just, under such circumstances, to use such expressions, or, indeed, to notice the matter at all ? If my ignorance on a subject of this kind, and under these circumstances, be scarcely to be credited, is it at all credible that Dr. James Carmichael Smyth should be ignorant of the very same experiments at the time of *his* employment at Winchester, when his honour and his character were so immediately involved in a faithful discharge of his duty ? If he was not ignorant of these experiments at the time when he was preparing to act under the conviction that nothing could so certainly or effi-

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caciously destroy contagion as mineral acids in a state of vapour, then must he be accounted a plagiarist ! Nay, more: if, knowing Morveau's experiments to have succeeded in destroying infection, and having himself been guided by the same experiments to the use of the same principle, for the same purpose, he afterwards ascribes his use of that principle not to those experiments, but directly and solely to his own deliberate reflections and his own unaided talents for discovery, then would Dr. Smyth stand convicted as an impostor ! He would be a plagiarist for borrowing that which he did not acknowledge. He would be an impostor for professing to have himself discovered that which in reality he had learnt from another man, whom he knew to have anticipated the discovery. I leave him, therefore, to take his choice between two distressing alternatives—between
 ignorance

ignorance equal to my own, or imputations of something worse than ignorance, from which he cannot escape, if he were not ignorant.

That my brother James did not owe his knowledge of the powers of muriatic acid gas to Dr. Priestley, I have proved ; that he did owe it to my father, he has acknowledged at a time when there could be no room for controversy, and even before Dr. J. C. Smyth's "attention had been called to this subject."

Dr. Smyth also reluctantly allows, that my brother's Treatise does evince that he "had made some approach" towards the discovery of a power that could certainly and efficaciously destroy contagion. It is remarkable that Dr. S. who takes infinite pains to inform his readers that he never heard of the name of my father, till an anonymous paragraph in the Morning Chronicle led him to enquire for his book, does *not* say that he never heard of the name of my brother James; who, the year

before Dr. Smyth was sent to Winchester, published a work on contagious fever, in which he recommends muriatic acid gas to correct vitiated air, and as the surest method of preventing the spread of contagion. In this book my brother, referring to the exact page of my father's Dissertation, in which the precept for raising muriatic acid gas is contained, says (not very accurately indeed, according to Dr. Smyth's expression), "that this method of correcting vitiated air was ordered long ago by my father." This is one of the boldest assumptions that ever was imposed on the patience of the public. What, *not accurately*, when he refers to the very page of the Dissertation of his father? Before any question about priority of discovery had arisen—Before Dr. S. had made his deliberate reflections at Winchester, my brother writes that his father had long ago ordered muriatic acid gas, and quotes a book published

published in 1758, to prove what he writes ; and yet it is daringly asserted by Dr. Smyth that it is not said accurately, although the marine acid gas will be found to be ordered at the page cited. This is another of Dr. Smyth's discoveries, and when he has established the grounds upon which he claims it, he may set up a canon of criticism well suited to maintain his grand discovery, and all future discoveries which he may *choose* to make, or which shall be found to proceed from a *like* deliberate reflection.

At p. 23 of his Letter to Mr. Wilberforce, Dr. S. introduces the mention of calamities which befel my family, in a way that renders it necessary for me to lay before the reader, at the same time my statement, and his remarks :

My Statement, p. 9, 12.

In 1776 Dr. Ed. Johnstone, now physician in Birmingham, was seized with putrid fever from attending some poor families

Dr. Smyth's Remarks, p. 23.

He informs us of two occasions where it (muriatic acid gas) was employed in his own family ; upon which I must needs

ies in Kidderminster. The muriatic vapour was kept rising continually in the room, and myself and the younger branches of the family were often officiously employed in stirring up the salt. My brother recovered almost miraculously from the last and worst state of this dreadful fever, and not one of the family, at that time consisting of fifteen or sixteen persons, was infected.

In 1783 a malignant fever broke out in the county gaol of Worcester. The keeper of the prison, his wife, and the surgeon who attended the prisoners, died; and so great and universal was the alarm, that attendance could not easily be procured. In this emergency Dr. James Johnstone jun. was called upon, and he died too, but not till he had stopped the progress of infection in the gaol. My brother was infected by too scrupulously

needs remark, that to state the cases of his two brothers, and a cousin, seized with the gaol distemper, as a proof of his father's knowledge of a certain method of preventing it, is a singular and new mode of induction.—He might with equal propriety and sound logic have attempted to prove that his father's antidote to contagion might be used with perfect convenience to the sick, from his declaration, that unless discharged at a distance from the patient, their extreme pungency would be offensive to the lungs. We are further assured by Dr. John Johnstone, that, owing to the marine acid vapour employed on those occasions, the fever was not communicated to the rest of the family. But the gaol distemper seldom spreads, or is communicated by an individual, when removed from the fomes
of

scrupulously discharging his duty; and, well aware of his danger, came to Kidderminster, into the bosom of his family, to have the constant attendance of my father. Human art was exhausted to save him, but in vain. All contagion was prevented from spreading in the family, which consisted of 12 persons, by the constant use of muriatic vapour. We had all access to my brother's chamber, and no one was materially affected except Major Johnstone; who probably caught the contagion by attending my brother from Worcester, shut up in a post-chaise, &c.

of contagion. Such instances afford at best but equivocal proofs of the efficacy of any means employed to prevent it.

I have put my statement directly opposite to Dr. S.'s criticism, that the readers may see at once the accuracy of his representation, the candour of his exposition, and the acuteness of his induction. He does not display his hardihood

dihood in contradicting it; he does not try his skill for candid exposition, upon the particulars of it. He just says that I had informed my readers of two occasions when it was employed in my family. The incautious or the inattentive part of Dr. Smyth's readers may suppose, that in his comment upon this information, he confutes the substance of what I had said about both cases. But his attempts to confute me are confined only to one of them, and upon that one he attacks me for a new and singular mode of induction, which in reality I did not employ. From the wariness, however, of Dr. Smyth in not dropping one syllable of observation or concession upon the other case, which was equally favourable to my side of the question, and equally unfavourable to Dr. Smyth's, I infer that the Doctor has most exactly conformed to a very useful rule, which a great writer of antiquity lays down in his *Precepta Altercationis*—"Ab iis quæ

" non

“*non adjuvant, quam mollissime pedem oportet
 “referre.”*: Quintil. lib. vi. c. 4. If Dr. Smyth ever read the foregoing passage he has profited by it very notably; and if he has not read it, his conduct exemplifies another observation made by the same author, “*similis est
 “arti, plerumque Natura,”* lib. viii., c. 4.

If I had stated that my brother and my cousin were seized with the gaol distemper, as a proof that my father knew how to prevent it, *then* should I have been guilty of the absurdity imputed to me. But the statement so confidently asserted, and so triumphantly refuted by Dr. Smyth, occurs in *his* letter, *not* in *my* pamphlet. Dr. Smyth will hardly deny that the first of the foregoing cases is accompanied with all proper notations of time and place, physician and patient, cause and effect. The time was 1776, or four years prior to Dr. Smyth's journey to Winchester—the place was Kidderminster, where my father re-

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fided—

sided—the physician was that father, the patient was his son—the cause of the disease was contagion, communicated to my brother Edward when he attended some poor families—the effects of the application of muriatic acid gas were recovery from contagious disease to my brother Edward, and exemption from contagious disease to the rest of my father's family.

My father had now been a practising physician for more than twenty years, and under the alarm which a domestic calamity excited in his mind, he would hardly have ventured upon the use of a medicine of which he had no distinct knowledge, no previous experience, no soothing suspicion, that it could be employed with safety as well as advantage. If Dr. Smyth, in a state of suspense or conscious ignorance, spared his patients at Winchester, Dr. Johnstone, under the same circumstances, may be supposed with the same solicitude to
have

have spared a son. But my father was happily qualified by his "deliberate reflections" for immediate and correspondent action. He discovered *more than an intention* to use the muriatic acid gas. He used it actually and successfully.

Again I stated upon another occasion, that my brother James was seized with the gaol distemper, and that a brother who was shut up with him in a post-chaise, had afterwards the local symptoms of contagion; that they came into a family consisting of twelve persons; that muriatic acid gas was then employed by my father; that James died; that Henry recovered; and that the rest of the family were not infected. James had been sent into a gaol full of contagion at Worcester, before the muriatic acid gas was employed, and being infected was removed to Kidderminster, where it was used; but from the progress of disease, without effect, so far as related to him.

Now, Dr. J. C. Smyth himself caught the gaol fever at Winchester, of which he has given a long, and, no doubt, faithful account, under the head, "Description of my own Case," (from p. 15 to 27 of his Description of the Gaol Distemper, &c.); in the whole of which Description *not a word is said of mineral acids in any shape*. Even in the experiments on board the Union hospital ship, notwithstanding the ship still continued to be fumigated once every day, contagion was not entirely destroyed, for one marine died, and one nurse had a slight attack of fever.* Thus, then, Dr. Smyth did not prevent contagion from reaching himself, notwithstanding all his previous deliberation and reflection; nor did the partial use of fumigation prevent the death of one man, even during the course of experiments. But let us apply Dr. Smyth's reason-

* Account of an Experiment, &c. p. 55.

ings more immediately to the case he has put. Did it therefore become impossible, because my father had discovered a method of destroying contagion in particular circumstances, that contagion should exist, and that my brother should be exposed to it? Does it not exist, notwithstanding all Dr. Smyth's deliberate reflections on the power of mineral acid gases? Did it cease to exist when fresh subjects were brought into the hospital ship, where his experiments were carrying on? If Dr. Smyth's deliberate reflections had taught him an effectual remedy and preventive of contagion, why was he infected at Winchester? I should have disdained to put these questions, had not Dr. Smyth insulted the memory of my father by an unfeeling and wanton sneer. For the physician cannot carry his censer in his hand wheresoever he goes, and he must always be exposed to the risque of contagion before he has had the power of taking those steps of
cure

cure or prevention which his skill may enable him to direct. Such was the case of my brother. He was called upon to perform a great public duty at a moment of emergency and alarm. The gaol fever was spreading in the castle of Worcester fatally. He caught the infection probably at his first visit, before it was possible to employ the mineral acid gases, and whilst he was giving those directions for employing them, with other remedies, which were to restore health and safety to others; and his death is now attempted to be made more bitter to us by the cruel reflection, either that my father did not know a certain power of preventing contagion, or that in the case of my brother it was not employed. That it was employed too late, and therefore without effect, we have all to lament. But I shall leave it to the soundness of Dr. Smyth's logic to prove, whether it be a singular and new mode of induction, to make an unqualified deduction
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of failure, where there was in the first instance no application. I shall leave it to his sagacity as a physician to determine, whether it be not possible for one who knows the power and the application of mineral acid gases, to be exposed to contagion and to be infected by it. And I will now leave it to any man of humanity, to assign the proper epithet to that critic and to that physician, who shall so far wrest the powers of reasoning and of science to an unworthy purpose, as to libel the fame of a father, by insinuating that the life of his first-born child, at least, was not saved by those means of cure which he pretended to possess; when the critic must know, from the statement, that the father did employ those means as soon as it was in his power to employ them; and the physician must know, if he has any experience, that these means when not employed early, may be tried in vain.

I quite agree with Dr. Smyth's remark, that
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the *fomes* of contagion must be much more dangerous than an individual case; and yet one man infected with malignant fever may communicate it to thousands. It is not possible to gain a more unequivocal proof of the benefit of a remedy, than that the effects which it was intended to prevent, were prevented.

It should be observed that my father approved and recommended the use of muriatic acid gas only in putrid diseases of the more virulent kind, when the danger from contagion was greatest. In the common scarlatina my father seldom employed it, unless the ulceration of the throat became the most threatening symptom. His great trust in the treatment of the general disorder was in acids and cool open air, which he considered as specific antidotes to the mischief of that poison.

In his remarks on the angina and scarlet fever of 1778, published in Vol. III. of the
Memoirs

Memoirs of the Medical Society of London, there is no mention of mineral acid gases; but there is an express reference to the successful use of bark, wine, acids, antiseptic diet, as a practice recently and fully described by his son James, and in his own Dissertation on Fever. It is impossible that he should forget the use of muriatic acid gas which occurs in both of them; and when the disease puts on its most malignant appearance he refers to his son's Treatise for ample directions how to cure it.

The leading object of my father's paper was to oppose Dr. Withering's distinction between scarlatina and angina maligna, and the method of cure by alkalis; which my father's theory led him to consider as favourers rather than correctors of putrefaction.

My father's knowledge of the muriatic acid, his skill in applying it commodiously as well

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as safely, and his confidence in its power to check contagion, increased, I am ready to allow, with his increasing experience; and for such experience there was ample room between 1758, when he published his book, and 1776 when he preserved my brother Edward and the rest of his family. His claims, so far as I am interested in supporting them, during this interval are endowed with the very best properties which an advocate for his cause could wish. *In loco opportunitas, in tempore longinquitas, in occasione commoditas ad faciendum idonea, in facultate copia et potestas earum rerum propter quas, aliquid facilius fit, aut sine quibus, omnino confici non potest consideranda est. Cic. de Invent. lib. 2.* His final and determined preference of muriatic acid to all the other expedients which his own practice, as well as that of his predecessors and contemporaries, had sanctioned, and his discontinuance of heat

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when the apartments were small, are intelligible and important instances of practical improvement upon his own discovery.

After all the trials which Dr. Smyth had made, and all the knowledge which he had accumulated in 1795, Dr. S. allows that future experience may improve upon what has been already done; and surely upon looking back to his own past experience he will find it to have been progressive.

If Dr. Smyth, when he was at Winchester, had really employed both the muriatic and nitric acid gas, if he had employed them directly and specifically for the purpose of destroying contagion, if he had employed them with those precautions which my father suggests, and the propriety of which Dr. S. cannot dispute, he must have found both of them safe and both of them useful; and he might then have also formed some opinion of their comparative safety and usefulness. But in what-

soever manner, and with whatsoever effect, either of them were used by him at Winchester, he, by his own account, had much to learn; much too he did actually learn from the experiments made with Mr. Hume of Long-Acre; and well does it deserve to be remarked again and again that these experiments were prudently made upon irrational animals, after the numerous opportunities which a prison and a hospital must have afforded in 1780, for ascertaining the effects of mineral acid gases on human beings. Yet if I do not misunderstand Dr. Smyth he then discovered, for the first time, that the muriatic acid gas was less respirable than the nitric.

Dr. Smyth infers that because my father mentions the sulphureous and marine acids together, that he was evidently unacquainted with any distinction in their application, and considered them as equally safe and equally useful. Were I to admit such a conclusion
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from such premises, it would still be apparent that my father had used the marine acid, or seen it used; that he knew the method of raising it easily, and that he discerned the necessity of raising it at a distance from the patient, and that the errors imputed to him about the sulphureous were incompatible with the judicious application of the marine. But I have already resisted the conclusion, because it is impossible for my father to have employed both the sulphureous and marine acids without perceiving a difference in them, without observing that in equal quantities they are not equally safe and equally useful, without knowing that in different quantities they could not be equally commodious, even if he thought them equally useful.

My father, in common with physicians, apothecaries, and nurses, was aware that burning sulphur had been used to drive out contagion; but if the use of it as a corrector of con-
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tagion ever formed a part of his practice, there is sufficient proof that he discontinued the sulphureous while he continued the marine. The sulphureous was not used in 1776 when my brother Edward was ill. It was not used in 1783, when my brother James died and my brother Henry recovered. It was not mentioned in 1779 when James published his Treatise. It was not mentioned in that year by my brother Edward, in his academical exercise at Edinburgh. It is not mentioned in my father's manuscripts. It is not mentioned in Mr. Crane's letter.

Some further evidence which my brother Edward supplied, and which I endeavoured to convey in the most unadorned and inoffensive terms, has been treated by Dr. Smyth with the dissingenuousness of a sophist, and the superciliousness of a scoffer. I shall therefore produce that evidence again, precisely in the same terms which I used before. I shall vindicate it from the objections which are raised against

it

it by Dr. Smyth, and I shall strengthen it with some additional matter which may convince my versatile and doughty antagonist, *Scuto sibi magis, quam gladio opus esse. Liv. lib. iii.*

“ In 1779 Dr. Edward Johnstone proposed this practice in the clinical ward of the royal infirmary, Edinburgh. It had not a fair trial, being used only one day, and then laid aside on slight grounds. He defended it afterwards in one of the dissertations for his degree before the professors of that university.”

Such were my words in p. 10 of my pamphlet. I shall now produce the answer made to them by Dr. Smyth: “ As for what he says of his brother, Dr. Edward, having mentioned the marine acid at Edinburgh in the year 1779, and that it was tried for one day in the royal infirmary of that city, it only shews that Dr. Edward as well as his brother, Dr. James, had, at the time, heard something

“ something concerning the marine acid from
 “ Dr. Priestley. But supposing the fact to be
 “ exactly as he has stated it, it would not im-
 “ press us strongly in favour of the marine
 “ acid vapour to know that it had been tried
 “ and rejected at Edinburgh, whilst Dr. Cul-
 “ len, the celebrated professor of chemistry,
 “ was at the head of the medical college and
 “ chemical wards; whom no one who ever
 “ knew him will accuse of having been an
 “ enemy to experiment or even to novelty.
 “ The inconvenience and danger, therefore,
 “ attending its use was probably the true
 “ cause of its being discontinued; and it was
 “ never afterwards, so far as I know, either
 “ tried or mentioned at Edinburgh, or any
 “ where else.” Letter, p. 24.

My observations on the preceding passage
 in Dr. Smyth's Letter will be employed; 1st,
 upon what it does contain, and 2dly upon
 what it ought to have contained and does
 not.

How

How far my brother James was indebted for his knowledge of the marine acid to Dr. Priestley has been already considered, and much of the reasoning which I then employed is applicable to my brother Edward. There is no evidence that Dr. Edward ever had any correspondence or conversation with Dr. Priestley on the powers of the marine acid, or that he had ever read the work which my brother James had read, and, in some respects, misunderstood. But unless error, like putrid fevers, be contagious, my brother could not learn from Dr. Priestley that the marine acid contains powers which Dr. Priestley neither expressly nor virtually assigns to it. He could not believe on the authority of Dr. Priestley that the marine acid was useful in destroying contagion, when Dr. Priestley in his letter to my father had complained of being misunderstood by my brother James, and had positively maintained that in his own opinion,

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and according to the result of his own trials so far as they had then gone, it had no such use. Dr. Edward had, indeed, in the year 1779 and before, learned something upon the subject from another quarter;—something true in theory and beneficial in practice—something resulting from medical, and therefore more important to him than deductions from chemical experiment—something which instructed him in the real efficacy of the marine acid to destroy contagion, and which pointed out to him the method of using it safely as well as advantageously. *That something* he had learned from his father. He could not but remember the use of the marine acid in 1776, for in that year his own life and the health of fifteen or sixteen persons in his father's family had been preserved by it. He could not but understand it, for he was then training up under the eye of his father for the medical profession, and in medical knowledge he had *then* made sufficient proficiency

proficiency to attend some poor families in Kidderminster, who were under his father's care.

In 1758 my father had published a Treatise upon Fevers, in which he mentions the use of the marine acid, describes the method of raising it, and gives proper precautions for employing it. Without any undue partiality as a son, or any extraordinary curiosity as one who intended to be a physician, Dr. Edward might have been induced to peruse that book. But if, upon the perusal of it, the same scruples had occurred to him which occurred to Dr. Smyth, about my father's opinion upon the *comparative* utility of the marine acid in 1758, those scruples must have been removed by events subsequent to that time: they must have been removed by his own experience, and by the experience of his father's family, when the marine acid was used with such favourable and unequivocal effects in 1776; they must have

been removed by the perusal of a book published by his brother Dr. James in 1779. I shall presently bring forward evidence, to shew that Dr. Edward *had* perused that book with attention and conviction ; and I shall now quote for a second time some passages from the book itself, to prove that he could not read it without knowing that such a thing as the marine acid existed ; that it possessed the power of destroying contagion ; and that his father having perceived in it that power, had employed it for that purpose. My brother James says, in p. 4 of his Treatise on the Malignant Angina :

That “ the ulcerated fore throat had for
 “ some years past very rarely appeared ; that
 “ in those few instances which did occur its
 “ malignity was much abated ; and that the
 “ management of it was become so familiar
 “ in many places that the good women themselves cured it by means of the bark and antiseptic steams.”

Again,

Again, in p. 6, he says, "antiseptic steams
 " composed of vinegar, myrrh, and honey, or
 " the acid air raised by pouring oil of vitriol
 " upon sea salt, constitute," together with
 other particulars which he enumerates in the
 same page, "the only successful method of
 " cure. This method applied early and with
 " due perseverance, has seldom failed."

Yet farther, in pages 110 and 111, he says,
 "As it is impossible too cautiously to guard
 " against the effects of so putrid a contagion,
 " the acid air, or spirit of salt, should be kept
 " rising continually in the room, by pouring
 " oil of vitriol once or twice a day on sea salt
 " placed in a convenient vessel. This spirit
 " will rise in the moderate degrees of heat
 " from 60 to 70 degrees of Fahrenheit's ther-
 " mometer, so as to be perceived in every part
 " of the room by its penetrating acid smell.
 " This method of correcting vitiated air,
 " which is useful in this and every other pu-
 " trid

“ trid disease, *was long ago ordered by my father*, and is now recommended by Dr. Priestley.

“ To have the patient’s chamber filled with these vapours is the *surest* method of preventing the spread of contagion; for which reason, as well as the patient’s particular advantage, care should be taken to preserve the air as pure as possible.”

So wrote my brother James in 1779; so read my brother Edward, as will be hereafter shewn, in that very year; and if he understood what he read, and believed what he understood, he must have known that the marine acid possessed the powers which are imputed to it, and that his father had long before 1779, called those powers into action, by ordering it for the benefit of his patients.

I produced the three preceding passages from the Treatise on the Malignant Angina before, in order to shew that my brother James derived

derived his knowledge of the marine acid from my father. I have produced them *a second time* in order to shew that my brother Edward derived part of his knowledge from the book of his brother James, and that he stood in no need of learning any thing from Dr. Priestley ; to whom *alone* Dr. Smyth most contemptuously and most rashly ascribes "*the something*," and by implication, *the whole*, which my brother Edward knew "when he proposed the practice of my father in the royal infirmary at Edinburgh."

My brother, Dr. Edward, might or might not have known that Dr. Priestley in writing to my father, had expressed very strong doubts on the power of the muriatic acid gas to destroy contagion ; but he must know that Dr. Priestley, even if he admitted that power, was a chemist not a physician, and therefore would have recommended rather than ordered. He must know that my father being a physician

was

was required to order; and that if he were an intelligent and an honest physician, he would not have ordered any powerful medicine, which upon *proper* occasions, he was not prepared to *recommend*. He must know, that my father in 1758 had *mentioned* at least the marine acid, had described the method of raising it, and had expressly permitted the use of it, whether he at that time, did or did not *decidedly* prefer it to the antiseptic steam of vinegar. He must know that subsequently to 1758 and long before 1779, my father *had given it that preference*, that he had ordered it in his practice, that he had recommended it to his sons James and Edward, that he had availed himself of it for the protection of his own family, and had even familiarised the use of it among his poor patients in the neighbourhood of Kidderminster. Knowing these things at this time, he knew more about the powers of the acid gases than Dr. Smyth appears to have known

known at the same time, or even at a later period. He knew *that*, which no well-informed physician, nor well-bred disputant would condescend to describe by the word *something*. He knew it *not* from Dr. Priestley but from his own experience when he was cured by his father, from the testimony of his own family when they were preserved by his father, and from his own reading when he looked into two books, one of which contains descriptions and directions of his father, about the marine acids, and the other of which shews that his father had long ordered it.

Dr. Edward *mentioned*, as Dr. Smyth says, the marine acid, or, as I say, “proposed a practice,” which included the use of it in the royal infirmary of Edinburgh. It may be presumed then, that the conviction of a young man must have been very strong, before he would *venture* to propose any new and active medicine, to instructors venerable from their

age, their experience, and their station. It can hardly be imagined that an opinion founded upon something he had heard about the marine acid from Dr. Priestley, would have emboldened Dr. Edward at all to *suggest*, or induced any of the Edinburgh professors at all to *permit* a single experiment, involving the use of a very powerful agent, and amounting to an alteration in the established treatment of putrid diseases in a royal infirmary. Whatever may have been the fondness of Dr. Cullen or his associates for novelty, and whatever their indulgence to experiments, they would not upon grounds so slight, have sanctioned a practice so dubious, in cases so dreadful. But my brother had weightier reasons than the something he is said to have learned from Dr. Priestley, for making the proposal, and weightier too than that something, were the reasons which led an Edinburgh professor even for once to accede to it.

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The marine acid then *was tried*, and it was also rejected at Edinburgh, "*whilst* Dr. Cullen, " the celebrated professor of chemistry, was at " the head of the medical college and chemical " wards; whom no one who ever knew him will " accuse of having been an enemy to experi- " ment, or even to novelty." Be it so: but I am not sure that the authority of Dr. Cullen will peculiarly bear upon this question, for Dr. Cullen was not at the head of the ward, nor indeed had any *practical* concern in the infirmary, when the experiment was proposed. Dr. Hope, not Dr. Cullen, was at the head of the ward. The trial was made with the permission not of Dr. Cullen but of Dr. Hope. The marine acid was laid aside with the knowledge and approbation not of Dr. Cullen but of Dr. Hope. My brother did mention it to Dr. Hope, but to Dr. Cullen he mentioned it *not*, nor is there any evidence that Dr. Cullen, though he was in the room when my brother talked with Dr.

Hope, attended to their conversation, or had ever heard of the proposal, the trial, or the rejection, from either of them, or any other person whatsoever.

The marine acid, as I said, was used only for one day, and therefore I stated that it had not had a fair trial, and had been laid aside on slight grounds. Surely Dr. Smyth, who now knows the power of the muriatic as well as the nitric gas to destroy contagion, though he holds the nitric to be more respirable than the muriatic—Dr. Smyth, who at Winchester had only sufficient confidence in the fuming nitrous acid vapour, to apply it there in small gallipots to the sick—Dr. Smyth who by his own confession, had erred in the means of procuring the nitric at the very time when he professes to have formed some intention of applying it, and who for many years after had met with no favourable opportunity of procuring it—Dr. Smyth who, in order to remove his
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own doubts, or to supply the imperfections of his own knowledge, was at some period subsequent to his Winchester journey content to make experiments upon two little animals, and by those experiments was enabled to discern the superior excellence of the nitric to the muriatic acid gas in point of respirability; and who, in recommending it afterwards for the cure or preservation of human beings, found it necessary to avert inconvenience or danger, by laying down the most explicit cautions for the use of it on board the Union hospital ship. Surely, after such gradual acquisitions of knowledge, with such a deep sense of the difficulty with which it is gained, and with such direct experience of the consequences which arise both from want of caution and want of perseverance, such a physician, or if he pleases, such a discoverer as Dr. Smyth, will readily allow a trial of one day not to have been *quite fair*. He will allow the inconveniences and dangers

dangers resulting from one experiment, not to be sufficient grounds for immediate and absolute rejection. He will allow that even if the nitric acid gas had been tried *without due precaution*, the *same* inconvenience and danger would have arisen, and that under the same circumstances, it in all probability, would have been attended with the same effect of discontinuance. In either case, however, the conduct of persons who, after one trial, and that perhaps reluctantly begun and incautiously conducted, had rejected the use of either gas, can have little weight against the more numerous, the more judicious, and the more successful experiments made upon the muriatic gas by my father, and upon the nitric gas by Dr. Smyth.

No man feels more veneration for the professional abilities, or more affection for the social virtues of Dr. Cullen than my father, and my brothers James and Edward; were
accustomed

accustomed to feel and to express. During my residence at Edinburgh it was soon my ill-fortune to be deprived of his most valuable instructions. I paid, in common with my fellow-students, a just tribute to his memory by putting on mourning; but I had not the honour of living with him upon terms of personal intimacy; and from his writings I am not warranted to decide upon the *extent* in which he was likely to be influenced by that sort of fondness, or at least indulgence towards experiment and novelty, which is ascribed to him by Dr. Smyth. In my general observations, however, upon the characters of men eminently distinguished by their talents and attainments, I have sometimes found that the love of novelty is most active, when the indulgence of it, is not adverse to some peculiar or favourite system. If, therefore, confiding in the sufficiency of his own theoretical opinions, and the soundness of his own practical rules

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for the treatment of putrid disorders, Dr. Cullen *was* inclined to undervalue the usefulness, or to exclude the consideration of the experiments made upon the marine acid by my father, I have only to lament that a very great and a very good man, was in one instance subject to an infirmity, from which Dr. Smyth himself is not entirely exempt. I think the discoveries of Dr. Smyth more remarkable for novelty in the means than originality in the principle. But I know that Dr. Cullen was not wanting in candour to acknowledge, or ingenuity to improve upon every hint which he caught from Hoffman; and were he now living to contrast the claims of Dr. Smyth with those of my father, I should repose the utmost confidence in the accuracy of his judgment and the fairness of his verdict.

The name of Dr. Cullen has been introduced *not by me* but by Dr. Smyth; but perhaps after all, the marine acid was neither
 tried

tried, nor laid aside, by his direction, or even with his knowledge. Be this as it may, I shall not retract my opinion that it was laid aside without a fair trial, and I leave my readers to decide upon the validity of the reasons which I have assigned for adopting, avowing, and retaining that opinion.

Dr. Smyth tells us, that “after being discontinued on account of the inconvenience and danger attending its use, the marine acid gas was never afterwards, so far as he knew, either tried or mentioned at Edinburgh, or any where else.”

Surely the limitation of Dr. Smyth’s knowledge as stated by himself, is no proof that the knowledge of my brother Edward was equally limited; and if the marine acid gas, though eventually discontinued, had been mentioned and tried at Edinburgh in 1779, an impartial reader will now be disposed to admit that the trial and the mention of it at *that* time, were

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the consequences of an intimation from my brother Edward, and that such intimation was the consequence of *something* which he had before heard and seen concerning the marine acid, not as recommended by Dr. Priestley, but as known to his father and his brother, and published by both.

What Dr. Smyth knew, or knowing might find it convenient to suppress, about the trial or the mention of the marine acid at Edinburgh or any where else, after the discontinuance of it in that city in the year 1779, I leave Dr. Smyth to determine. His ignorance may, or may not, be real ; it may be even venial, if real. But it is no measure for the knowledge of my brother Edward, and therefore has no weight in counteracting his testimony. It is possible, and I add certain, that previously to the discontinuance of the marine acid at Edinburgh, and subsequently to it, he knew *facts* which Dr. Smyth did not know—

that

that he had seen trials which, in 1780, and, so far as appears, before 1795, Dr. Smyth had not seen—that in cases of disease unknown to Dr. Smyth, at times unknown to him, in places unknown to him, and before persons unknown to him, my brother might *himself* have mentioned the use of the marine acid, and himself have used it, though a total silence about it, and a total disuse of it, prevailed at Edinburgh. It is probable, too, that in conversing with professional men about his father's practice and his own, he might have heard the muriatic acid mentioned by *other* persons, for other persons have long known the practice of both; though the evidence of my brother is rudely slighted by Dr. Smyth, and the name of my father is said not to have been even heard of by him till 1802.

I shall now proceed to the consideration of that part of my brother's evidence which Dr. Smyth ought to have mentioned, but does not;

and in so doing I shall have occasion to introduce the additional matter which has been already noticed.

In the passage some time ago quoted from p. 10 of my pamphlet, it is said that “my brother, who had proposed the use of the marine acid in the clinical ward* of the royal infirmary, Edinburgh, where it was used only one day and laid aside upon slight grounds, defended it afterwards in one of the disputations for his degree before the professors of that university.”

Dr. Smyth, who, in his remarks on the thesis of my brother James, expatiates on the omission of the marine acid in that thesis, shews a want of attention or a want of candour in passing over a Latin Dissertation for a degree, in which the use of the marine acid *was* mentioned and defended by *another* brother.

* I must here acknowledge a mistake: it was not specially the clinical ward, as will be seen hereafter.

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The defence itself might be unsatisfactory to the professors. But if my brother entered upon that defence when the muriatic gas had been tried and laid aside, his conviction must have been firm, though his arguments had unfortunately been inconclusive; and at all events he cannot be supposed, without such conviction, to have undertaken the defence of a principle which he was not impelled by vanity to hold forth as a discovery of his own, and which, in the very moment of defending it, he believed to have been the discovery of his father. Such puerile levity, such unwarrantable audacity, such unprofitable wantonness must not be imputed to a young man, who was speaking in the presence of his seniors and his instructors, and whose credit depended not upon their assent to his opinions, but their approbation of his exertions. Upon this subject, however, little room is left for cavil to my opponent. I have asserted that my brother defended the practice
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of *his father* : I shall now endeavour to support and illustrate the assertion ; and whatsoever impression I may make upon the understanding of Dr. Smyth, yet contending for a plain matter of fact, I shall assume, without reserve and without apology, that the delicacy becoming Dr. Smyth's profession, the prudence becoming his age, and the diffidence becoming his cause, will alike prevent him from trifling with the veracity of my brother Edward, and my own.

The question upon which my brother drew up his academical exercise now lies before me in the hand-writing of Dr. Hope, and runs thus :

“ *Juvenis 18 annos natus, die Mensis 20 sub diodum plueret versatus, post reditum rigore et lassitudine correptus fuit.*

21. *Lassitudo cum capitis dolore.*

22. *Calor pulsus frequens, dolor capitis magnus ; sanguis parvâ quantitate e naribus stillavit.*

vit. Emeticum sumpsit sine symptomatum levamine vel ullius in stomacho cruditatis indicio.

23. *Caput dolet multum, adsunt calor fitis cum pulsu frequenti debili: in vultu hebetudo & tumefactio, adest delirium cum tendinum ad carpum subsultu, os saliva scatet quam copiose expuit. Alvus est stricta. Sudor perpetuus urget. Quær. morbus: prognosis: ratio symptomatum: Indicationes curæ et remedia:*

“Do. JONSTON,

31 May 79, prop. J. H.”

In discussing the foregoing subject my brother took occasion to mention and to justify his father's use of the muriatic acid.

Dr. Edward lays no stress upon the merits of his academical exercise as a composition. But, in a discussion upon the claims of his father, he considered it as a testimony which, in
some

some degree, supported them, and in consequence of Dr. Smyth's letter to Mr. Wilberforce, he endeavoured to obtain it, and wrote to Dr. Gregory in the following terms :

DEAR SIR,

Presuming on the kindness I received from you during my residence at the university, I take the liberty of troubling you on a subject peculiarly interesting to myself and my family. Among many other marks of your attention, you did me the honor to appoint me your clinical clerk during the latter period of the session 1779. You may perhaps recollect, that immediately after the course of clinical lectures was closed, the ward appropriated for the men was filled with soldiers, belonging to the duke of Buccleugh's fencibles, affected by a contagious fever. Having experienced the good effects of muriatic acid vapour in obviating contagion, under the direction of my father at Kidderminster, I recommended it to
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the late Dr. Hope, who immediately ordered it to be used, but in a short time, I think the next day, discontinued it.

Among the exercises for a degree Dr. Hope gave me the case of one of these soldiers to comment upon. In my commentary I stated that my father had long and successfully used the muriatic acid vapour to prevent contagion. Dr. Hope very candidly explained that his reasons for discontinuing it were—aggravated cough, and some pneumonic symptoms which attended that fever; and, upon consulting Dr. Bell's thesis, I find these are regarded as constant symptoms. I then stated my own experience of its use during a violent typhus caught by attending the poor at Kidderminster in the spring 1776, and that not one of a very numerous family was infected by me, which was attributed to the vapour being constantly raised in the room, and that I did not perceive the least inconvenience from it. You and the late

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Dr.

Dr. Cullen were in the library during my examination; but I can scarcely expect that after a lapse of near 26 years you can remember what then passed; and indeed my dear Sir, I have only to request that you will be pleased to get a proper person to look over the infirmary-book for May or the beginning of June 1779, and if any report is made respecting the use of the muriatic acid vapour, to procure me a certified extract of it, and likewise to get me a certified copy of my commentary on the case given me by Dr. Hope, which I believe, agreeably to the rules of the university, is deposited in the library, and I shall, with gratitude, discharge any expence you may incur. If you have seen my brother's essay asserting my late father's claims to the discovery of the effects of mineral acid vapour in destroying contagion, and Dr. C. Smyth's answer, you will perceive the necessity of my procuring these documents. My brother, who originally took
up

up the business, is preparing an answer to Dr. C. Smyth, which I will send you when published.

I am, dear Sir, with the greatest esteem,

Your obliged and faithful servant,

E. JOHNSTONE.

Lady-Wood, near Birmingham,

February 24th, 1805.

I shall subjoin the answer which my brother had the honor of receiving from the very learned Dr. Gregory :

Edinburgh, Saturday evening,

13th April, 1805.

DEAR SIR,

You must, no doubt, have wondered much at my delay, and thought me very negligent in not sooner answering your letter of Feb. 25th—but do not condemn me unheard. I lost no time in making the proper inquiries at the college library by means of the librarians,

and at the royal infirmary by means of Dr. James Hamilton, the senior physician of it, whom probably you will remember. He has been very obliging on this occasion, and has taken a great deal of trouble about it, but hitherto in vain. He can find no vestige of the use of the muriatic acid vapour about the time and in the manner that you mention. But again and again he begged of me not to write to you till he should have made further researches. Nay, even this day, when he told me of his hitherto bad success, he wished me not to write to you, as he is resolved, as soon as he can command two or three hours for the purpose, to make one trial more. The truth is, the register of the infirmary has been very irregularly kept; and many of the original report books have *been stolen* before they were quite finished, or *could* be transcribed into the register.

As to the *written exercises* of our candidates, I
could

could have told you, from distinct memory, that *about* or *before* the year 1779 the practice of *binding* them up, and *keeping* them in the college library, was given up for very substantial reasons. If I remember right, some of those volumes, formerly bound up and kept in the college library, were *cut up*, and the several professors took away the *cases* which they had given out, and the commentaries written on them. These kinds of exercises have never since been kept. But not being sure of the *precise time* of that change I inquired at the under librarian's, who assured me there were no such MS. volumes in the library, unless they were in some of the locked chests, the keys of which were kept by Mr. Profr. Dalzel (who succeeded Rabbi Robertson as principal librarian). Mr. Dalzel was at that time, and indeed is still, so much indisposed as to be unable to go to the library; but he assures me (and he is one of the most accurate men I

ever

ever knew) that there are no such MS. in the locked chests, the contents of which he knows perfectly ; and that there have been no such MS. volumes in the library in his time. All this I could have told you near a month ago, but I waited day after day for Dr. Hamilton's final answer. If he shall even yet find the record which you inquire after, I shall without delay, send you an authentic copy of it. But the chance of this seems to me very small.

Your's most truly,

J. GREGORY.

According to his own views of propriety and convenience Dr. Smyth will account for his silence about my brother's academical exercise, which had been mentioned in my statement as *one* of the evidences tending to support the claims of my father; and on the perusal of Dr. Gregory's letter he will find that my brother Edward is not responsible for the probable loss
of

of a document which I should have gladly produced, if he had been fortunate enough to obtain it. In the mean time he will give Dr. Edward credit for memory enough, not to be *grossly* mistaken upon the plain and substantial facts of having proposed my father's practice in the infirmary, and defended it in his exercise; for veracity enough, not to have invented a story about circumstances which never existed; and for prudence enough, not to appeal to a document which must have been injurious to his honour, if, upon being found, it had contained *no* vestiges of the muriatic acid, as used by my father.

I know not what reliance the candour or the condescension of Dr. Smyth will induce him to place on the mere accuracy of Dr. Edward's recollection. But if he will concede to Dr. Edward even a small portion of common sense, he will find in the letter written to Dr. Gregory very clear evidence that my father knew
and

and employed the marine acid gas at least in the year 1776 ; that Dr. Edward makes no mention of Dr. Priestley ; that he does not controvert, at least, my late father's claim to the discovery of the effects of the marine acid in destroying contagion ; that he had himself experienced the good effects of it ; that he had mentioned those effects to the late Dr. Hope, recommended the use of it in the infirmary, and in an exercise for his degree had stated that his father had successfully used the muriatic acid vapours to prevent contagion. If the exercise had been found, his evidence probably would have been fuller and more distinct. It so happens, however, that having been induced by Dr. Smyth's letter to look over his papers, he lately found one which may in part supply, what the exercise contained. That paper is now before me in the hand-writing of my brother. It is written in English, and was prepared for the purpose of
being

that when the disorder was ascertained I could easily foretell what plan he would direct in case of putrid fever, which used to be very prevalent in this town. He always directed the fumigation of oil of vitriol and salt, the patient to be washed in vinegar and water, with bark and mineral acids according to circumstances, and plenty of fresh air.

In 1776 a fever broke out in the workhouse here; he ordered the muriatic vapour, and his usual plan.

In 1784 a fever broke out in the workhouse here; he ordered it again, and was thanked by the town. Indeed I believe there are few cases on record in which practitioners have been more successful in curing fevers than Dr. Johnstone, of which this town and neighbourhood are sufficiently grateful in their remembrance.

I know not what Dr. Smyth means by *ex-parte* evidence; it is impossible that he can

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presume

presume to deny facts which he has not investigated, and which I am ready to stand forward to substantiate.

I remain, dear Sir,

Your obedient servant,

(Signed) JOSEPH CRANE.

(Directed)

Dr. John Johnstone, Birmingham.

In my pamphlet I endeavoured to relate facts nearly in their chronological order. But the laws of controversy are more complex than those of statement, and therefore that order has been disturbed, by the necessity imposed upon me sometimes to adopt the arrangement which Dr. Smyth found most convenient in his Letter to Mr. Wilberforce, and sometimes to step aside into collateral discussions, which the cavils or the revilings of Dr. Smyth compelled me to pursue. It will, therefore, be proper for me, in this place, to recapitulate the

various

various grounds upon which I have endeavoured to vindicate my father's claim to the prior knowledge and prior use of mineral acid gases to destroy contagion. These claims rest upon my father's Dissertation on Fevers, which, if controversy had been foreseen, would doubtless have been more perfect in some respects, and which Dr. Smyth depreciates in many others, in which it is apposite and important. Upon the paper of my brother James on different kinds of air, which was sent by my father to Dr. Priestley in 1775, and is now in part published for the first time. Upon the medical treatment of my brother Edward's fever in 1776, which Dr. Smyth slightly notices, without an attempt to controvert the relation which it bears to our dispute. Upon the Treatise on Malignant Angina, which was published in 1779, by my brother James, and in which he expressly states what had been long the practice of my father.

Upon my father's manuscripts, in which, after paying a just tribute of praise to Morveau for his subsequent discovery of the same principle in the same agent, and to Dr. J. C. Smyth for his successful application of that principle to another agent, he deliberately and unequivocally declares himself to be an inventor. Upon my brother Edward's recommendation of my father's practice to the royal infirmary of Edinburgh, in 1779, his mention of it in an English paper, and his defence of it in a Latin academical exercise, during the same year, in the same university. And, finally, upon the testimony of Mr. Crane, which is too pertinent to be slighted, too explicit to be evaded, and too decisive to be refuted.

In conformity to the intention which I intimated to the reader, by a note at p. 2 of this Reply, I shall now proceed to examine Dr. Smyth's own claim somewhat more minutely.

How

How did Dr. Smyth employ the mineral acids in a state of gas at Winchester in 1780? Upon that use is founded his earliest claim. "In the year 1780, (says Dr. Smyth,) the three mineral acids were employed by me in the prison and hospital at Winchester, and my opinion of their superior efficacy for destroying contagion was communicated by letter to the Board of Sick and Hurt, and mentioned publicly by me on many occasions," p. 25. When Dr. Smyth brings testimony of the employment of mineral acids in a state of gas in 1780, before the committee of the House of Commons, the only evidence tending to prove that fact is that of Robert Lulman, Esq. "He saw the nitrous acid used. The vessels in which it was contained were placed on the floor, between the patients' beds, and in other parts of the wards, and the good effects were soon apparent, as described in Dr. Smyth's publication. The nitrous acid was used,

“ used, in a fuming state, while the patients
 “ were in their beds, and was kept there night
 “ and day; nor did he see or hear any com-
 “ plaint of its producing suffocation or diffi-
 “ culty of breathing to the patients or attend-
 “ ants. Dr. Smyth’s statement of these pro-
 “ ceedings, as given in his work, is perfectly
 “ correct.” Report from the committee on
 Dr. C. Smyth’s petition, Ap. No. 2.

“ Dr. H. R. Reynolds, physician to his
 “ Majesty, stated that he never heard of
 “ the nitrous fumigation till it was suggest-
 “ ed by Dr. Smyth, previous to his going
 “ to Winchester, when they were colleagues
 “ together at the Middlesex hospital; and
 “ he never heard of its being put in prac-
 “ tice till his return from Winchester,” &c.
 Appendix, No. 25.

This is the whole of the evidence brought
 before the committee, even for Dr. Smyth’s
 use of *fuming nitrous acid* in 1780. Dr. Smyth’s
 letter

letter to the Board of Sick and Hurt, to which he alludes in his Letter to Mr. Wilberforce, is not mentioned in the report. There is no evidence of his having publicly mentioned it, except to Dr. Reynolds, and no use of it in hospital or private practice is even alluded to; on the contrary Dr. Reynolds, his colleague at the Middlesex hospital, says, expressly, that he never heard of its being put in practice till his (Dr. Smyth's) return from Winchester.

His own book, had it been published in 1780, and the letter he wrote in 1780, would have been satisfactory testimony; but the letter is not forthcoming, and the book, we may suppose, received some little bias from the corrections and improvements which would probably take place between 1780 and 1795, in which last year it was published. The interval between my father's use of muriatic acid gas at Kidderminster, accompanied by all proper precautions, and the publication of his Historical

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cal Dissertation, was two years, from 1756 to 1758. The interval between Dr. Smyth's use of *fuming nitrous acid* and the publication of his account of the Winchester fever, was about 15 years, from 1780 to 1795.

In his letter Dr. Smyth says, "the three mineral acids were employed by me at Winchester." Now, the question is not, whether the acids in their usual state were employed, but whether they were employed by Dr. Smyth in a state of gas. In his work on Winchester fever, the use of them is to wash the bed-posts, &c. with diluted muriatic acid, to burn brimstone, and to burn nitre in the wards, and to expose gallipots three parts full of fuming nitrous acid in them. This is all the use of the mineral acids mentioned by Dr. Smyth, as used by him at Winchester, though he proposes a better plan before he closes his book. Let it be remembered that this book is not published till the autumn of 1795, a
year

being delivered before the medical society, where for some reason or other Dr. Edward had no opportunity of producing it ; and it is represented by him to me, as containing much of the matter which he introduced in his Latin exercise.

I shall make the following quotations, because they will shew the purpose for which the paper was drawn up, the subjects to which it relates, the time at which it was composed, and the methods of cure which Dr. Edward then knew to be employed by his father and recommended by his brother James :

“ MR. PRESIDENT,

“ As we have lately had an opportunity of
 “ observing the progress of a very virulent con-
 “ tagious putrid fever, I shall beg leave to offer
 “ a few hints on that subject to the society.”

P. 1.

“ Acids are excellent remedies as antiseptics,
 “ whether vegetable or mineral. The vege-

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“ table

“ table have been recommended as much pre-
 “ ferable to the mineral, from a supposition
 “ that the latter only pass through the intesti-
 “ nal canal without entering the system. But
 “ I must confess that I am very doubtful of
 “ the fact, and from the tonic power of the
 “ sp. vit. I am much inclined to give it the
 “ preference to any.” P. 19.

“ The patient should be kept in a large
 “ room, where the air is free, and in which the
 “ putrid effluvia should be corrected either by
 “ the steams of vinegar kept boiling in it; or,
 “ what has been found *to answer much better*,
 “ the acid air of spirit of salt kept rising in the
 “ room by pouring oil of vitriol once or twice
 “ a day on sea salt placed in a convenient ves-
 “ sel in the room. An objection to this has
 “ been made, that it is too acrid; but though I
 “ have *frequently* had an opportunity of seeing
 “ it tried, yet I have never found the patients
 “ complain, except just at first, when they were
 “ not

“ not used to the smell ; *and my father has long*
 “ *used it* in common practice, and assured me
 “ of the same thing.” P. 21 and 22.

“ For many hints on this subject, *i. e.* the
 “ use of the antiseptic plan, I would recom-
 “ mend a treatise lately published on the An-
 “ gina maligna, by Dr. Johnstone of Worcester,
 “ from whose work I have taken many hints,
 “ and whose method of cure I frequently had
 “ an opportunity of seeing the success of.”
 P. ult.

In the 3d quotation from my brother's paper we see not only the positive but the comparative utility of the muriatic gas, as long and safely used by my father in his common practice. In p. 21 of the Letter to Mr. Wilberforce, Dr. Smyth makes a distinction between my brother James's assertion that the acid air, as Priestley calls it, or the muriatic acid gas, was ordered by my father, and his not saying that he had known him use it, or that he him-

self had employed; or ever seen it employed by others; and to this frivolous and captious distinction he immediately subjoins an unqualified declaration that Dr. James "was evidently without experience on the subject, "equally ignorant of the powers of the marine "acid; and of the inconvenience which an "imprudent use of it might occasion." If Dr. James learnt nothing more from his father's book, he might, at least, have learned the inconvenience which an imprudent use of the marine acid might occasion. He might also have learned the power which, together with other mineral acids and the steams of vinegar, it possesses of medicating the air which the patient breathes. He might have learned a more sure way of raising it than Dr. Smyth, by his own confession, knew for raising the nitric acid gas, when he was at Winchester. Even with such knowledge we may presume that he was not wholly without experience:

but

but his book sufficiently refutes the rude imputation both of inexperience and of ignorance. What my brother's practice was, and from what sources he derived the knowledge which led to it, may be seen in his treatise: but the paper written by my brother Edward is of use, as containing additional testimony to the reality and the efficacy of it; and testimony I will add, which is not defective in one very material condition prescribed by Dr. Smyth. Dr. Edward frequently had opportunities of *seeing* the success of his father's practice.

In p. 26 Dr. Smyth tells us that his treatise on the Winchester fever was sent to Birmingham, where Dr. Edward Johnstone is, or was, physician: that public trials were made with the nitrous vapour at that place, by order of Dr. Withering, and that the result of them was published in Dr. Duncan's Annals of Medicine. Dr. Edward now is, and for more than twenty years has been, a physician at Birmingham;

mingham; where, without the indulgences of courtesy or the bounties of panegyric from Dr. Smyth, he is likely to retain that esteem which his professional abilities have long procured for him among his employers, his medical brethren, his neighbours, and his friends.

In regard to the passage where Dr. Smyth speaks of the trials made by Dr. Withering, my brother has favoured me, in writing, with the following communication: “ I did not
 “ know of Dr. Withering’s experiments on the
 “ nitric vapour, nor had I ever heard of his
 “ trying it. I never have tried the nitric va-
 “ pour in my practice, because I had expe-
 “ rienced the good effects of the muriatic acid
 “ gas both in my own case, and that of others;
 “ and when I first read Dr. Smyth’s book it
 “ appeared to me that my father’s method of
 “ raising the muriatic vapour was far more
 “ simple and convenient than Dr. Smyth’s
 “ mode of raising the nitric acid vapour; in-
 “ dependent

“dependent of my conceiving that the taste
 “and sensations produced by the former were
 “far more agreeable and less inconvenient than
 “the effects of the nitric acid gas.”

I am thankful too that I can have the living testimony of a man whose good sense, professional skill, worth and respectability, well entitle him to public confidence. The following letter I received from Mr. Crane, surgeon in Kidderminster, on presenting to him Dr. C. Smyth's Letter to Mr. Wilberforce :

Kidderminster, April

2, 1805.

DEAR SIR,

I thank you for a sight of Dr. Carmichael Smyth's letter, and so far as it relates to me, truly I do rejoice that I am enabled to bear further testimony to the discovery of your father, my old and confidential friend. I settled in this town in the year 1775: at that time I well remember that Dr. Johnstone constantly used (by pouring oil of vitriol on common salt) the
 muriatic

muriatic vapour in all cases of putrid fever; and I was informed by Mr. Cooper and Mr. Symonds, who had known Dr. Johnstone's earlier practice, that he had used it soon after he settled in this town, and that it always continued to be his practice. It was indeed so well known to be useful in destroying putrefaction, that thirty years ago, the manufacturers were accustomed to place it in their shops, of their own accord, as I believe I stated to you before.

From my first professional acquaintance with your father I have never known any serious putrid disorder in which he did not use the muriatic fumigation, and in which we had not been taught by him to rely on it as the chief corrector of putrefaction: indeed from my first knowledge of Dr. Johnstone to his last month, and I was employed almost constantly with him from the year 1775 to four weeks previously to his death in April 1802, his practice in fevers continued so invariably the same that

year after Guyton de Morveau had published his "*Instructions sur les Moyens, &c.*" when the muriatic acid gas was ordered to be used in the hospitals of the French armies. From 1780 to 1794 the result of Dr. J. C. Smyth's reflections, so important to mankind, so important to his own honour as well as his emolument, lies asleep; nor is it awakened to life till Dr. Morris, in 1794, applies to him for advice. Then he starts up, and his recollection being probably refreshed by Morveau's wide and notorious use of the muriatic acid gas, he bestirs himself, either to discover some means upon the same principle but not the same agent, or to ruminate upon his old *intention* to discover some such agent. With this view he fixes upon the nitric acid gas; perhaps having discovered at Winchester that fuming nitrous acid does not furnish a respirable gas, and knowing that in the use of the muriatic he had been anticipated by Morveau, and possibly by

others. The nitric acid gas he did *not* use, and according to his own account, he *could* not have used at Winchester in 1780, for gallipots of fuming nitrous acid would give out suffocating *yellow or red fumes only*. Nay, Dr. Smyth confesses himself mistaken in the principle on which he burnt the nitre, and as to the fumigations of brimstone, as well as the deflagration of nitre and gunpowder, they were in common use long before, as drivers out of contagion, *not* chemical correctors of it. There is no evidence, then, from his book, or from the testimonies of Mr. Lulman and Dr. Reynolds, that Dr. Smyth was acquainted with the principle in 1780, since, if we may believe his own statement, he did not employ *either* of the respirable mineral acid gases. He employed fuming nitrous acid I admit, and he employed it from a previous experience, “that the fumes
 “ of nitrous acid did not affect the breathing
 “ in the same manner (as what?) as the fumes
 “ of

“ of sulphur, which are so successfully made
 “ use of to fumigate clothes or furniture,”
 p. 55.

Now, in regard to Dr. Smyth's comparative feeling of inconvenience from the fumes of sulphur, or of fuming nitrous acid, it is entirely personal, and if he can at all bear fuming nitrous acid, I may say peculiar. To my perception fuming nitrous acid is nearly as intolerable and suffocating as burning sulphur, (*cæteris paribus*) and I cannot but conceive, whatsoever may be the peculiarity of Dr. Smyth's own sense of its impression, that his patients “ exposed to the nitrous fumigation in hospital
 “ wards, and in the apartments of the sick,
 “ without perceiving any unpleasant effect
 “ from it,” must have been exposed to it at a very considerable distance, and in such a diluted state, as for it to be nearly imperceptible, and therefore, in all probability, not very efficacious. From all that appears in his book, Dr.

Smyth made his first experiments with fuming nitrous acid on his patients ; but when he determines “ to render the experiment still more “ conclusive,” after exposing a mouse and a greenfinch, he then exposes himself, and his friend Mr. Hume, (ourselves) to the fumes of nitric acid, “ obtained by mixing nitre with “ heated vitriolic acid.” Here is the beginning of the true method, but we are not informed when it was employed ; the consummation in medical practice, certainly took place not till November 1795 on board the Union hospital ship. It will naturally occur to the reader, did Dr. Smyth know of Morveau’s experiments at this time ? He confesses he did, and the progress of his mind was from doubt to conviction, from very imperfect to perfect knowledge, from intention which he first formed, then distrusted, and then laid aside, to direct and plenary execution.

In his memorial to Lord North, in which

Dr.

Dr. Smyth enumerates his services, as furnishing a claim to remuneration, he says not a word of his use of mineral acids, or of his discovery, or of any principle for destroying contagion, even though he details his plan of cure at some length. Had he thought himself a discoverer in 1780, as he professes to have thought in 1802, would he not have mentioned it immediately after? Is it possible that he could forget such a plea, whilst his mind was intent on reward, and in the act of accumulating all his merits and all his sufferings, for the purpose of presenting them to the eye of his patrons and the public? If Dr. Smyth did verily believe himself to be the discoverer of the power of mineral acid gases to destroy contagion, he is the first suppliant for reward I ever heard of, who forgets in his petition, his *most* meritorious service; the first memorialist who neglects, in his memorial, his *greatest* act of benefit; a benefit not confined

to

to the particular office in which he had been employed, but extending to all mankind. For I appeal to Dr. Smyth's memorial itself, which is printed in his volume on the Winchester Fever, and which does not hint at the discovery of any chemical power to destroy contagion, but describes the use and well-known effects of *purification* only.

In p. 24 of his Letter to Mr. Wilberforce, Dr. Smyth tells us "this supposed discovery (*i. e.* my father's) "remained* a dead letter

* That Doctor Smyth's critical powers may not be roused into action by this various reading, I must acknowledge that the word printed in the Doctor's book is "remaining," but the sentence would not then be grammatical; therefore *meo periculo*, I have restored the true and original reading, on the presumption that the blunder was not made by my author but his printer. After this explanation, I hope that my "talents for quotation" will not be again arraigned.

As Dr. Smyth has taken upon him to school me upon my use of the English language, I shall beg leave to pay the *Spentio* due to my instructor, by recommending to his consideration a few passages which are not strictly conformable to general grammar,

“ for fifty years, and Dr. John Johnstone,
 “ who now comes forward as the claimant for
 “ his father, and champion of his family, was
 “ for twenty-three years, at least, silent on

grammar, or to the idiom of what Englishmen call their vernacular tongue :

“ I embrace the first moment,” p. 1—we say, embrace opportunities, not moments. “ You was chairman,” p. 1—since the publication of Lowth’s grammar, men, women, and children, have been taught to write “ you were,” p. 10. “ This caution flatly contradicting the son’s assertion.” If Dr. Smyth recollects that full stops are placed at his own word paragraph, and at the quotation ending at the word lungs, he will find that contradicting ought to be contradicts. P. 13 “ set it at rest for ever”—if the word “ it” refers to pretended discovery, Dr. Smyth would have written more correctly, by saying, “ set the question about it at rest.” P. 31, “ the preceding authorities *is* the only answer.” I cannot guess for what particular purpose of rhetorical emphasis, the monosyllable *is*, has been decorated with italics. But I believe the generality of men of letters would have written, are. After all, these petty verbal imperfections have little to do with the merits of our controversy, and I have pointed them out with the design not to annoy Dr. Smyth, but to check the eagerness he displays, to insult me.

“ the

“ the subject.” Now, during these fifty years, my father published one book, my brother James published another, my brother Edward proposed the practice in the university of Edinburgh, and defended it in one of the public disputations for his degree. All three of them ordered it in their practice. My father had given it so much notoriety in the neighbourhood of Kidderminster, that the poor people were able to apply it for their own protection; and in order to bestow upon it yet more useful and more extensive notoriety, he had communicated it to Dr. Priestley, with the hope that it would be presented to the Royal Society, and printed in their transactions, which are circulated throughout Europe. The discovery of the muriatic acid gas, therefore, was *not* “ a “ dead letter,” unless the absence of an application to Parliament for reward be the specific property, which in Dr. Smyth’s apprehension, subjects any discovery to that name, theoretical

or

or practical, medical or chemical. My silence, it seems, continued only twenty-three years, in which the *terminus a quo*, is Dr. Smyth's journey to Winchester in 1780, and the *terminus ad quem*, is the publication of my pamphlet in 1803. With all due submission to Dr. Smyth's accuracy in arithmetic, I was not quite silent for quite twenty-three years. According to Dr. Smyth's own account, I had sent a paragraph to the Morning Chronicle of March 12, 1802, and finding (as the Doctor *conceives*) that this paragraph did not attract any notice from physicians or from Parliament, "I came forward *propria persona*, by a letter addressed to " Mr. Wilberforce, as chairman of the committee, and I employed the interposition " of my friend, Sir William Pulteney, who " brought for (the committee's) inspection " the father and brother's publications," alias *dead letter*. To be sure I was thus far guilty of silence, as not to proclaim, by any publication of my own, the discovery which, in my

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judgment,

judgment, had been proclaimed before by my father and my brother James to that public ; and I broke my silence to Parliament, because Parliament had been recently appealed to by Dr. Smyth, and the object of my appeal, was to secure my father's claim to the prior discovery of a principle, which then engaged the attention of Parliament.

Let me now, in my turn, be permitted to ask, why Dr. Smyth was silent for a very long time about his discovery? For about the discovery he certainly was silent in his memorial to Lord North ; he was silent upon the same subject, though communicative enough upon another, and very distinct subject,* in his conversations with Dr. Reynolds ; he was silent to the public till 1795 ; he was silent to Parliament till 1802 ; and yet from 1795 to 1802 he most assuredly was, from his own personal

* I must again and again entreat the reader to observe the distinction between the use of fuming nitrous acid and of nitric acid gas.

experience,

experience, acquainted with the principle and the use of the respirable mineral acid gases. I suppose that he broke his silence to Parliament when he found a favourable opportunity for breaking it in support of his own claim; and I did the same when a proper, and indeed the first, occasion arose for my endeavours to preserve my father's claim to priority from encroachment. I did not mean to interfere with Dr. Smyth's great and merited reward for the application of the nitric acid gas, but I did mean to give Parliament an opportunity of bestowing that reward in such a manner, as should do no injustice to my father's claim to the prior discovery and application of the principle, in muriatic acid gas.

In what manner, and to what extent, were the deliberate reflections of Dr. Smyth employed from the time of his returning from Winchester to the time of his making experiments on birds and men, jointly with Mr. Hume of Long-Acre? The Doctor was during this

time, a practising physician in the Capital. He was also physician to an hospital. Let me ask, then, whether among his patients, there did not occur some putrid diseases in this interval? Let me ask, whether he repeated among them, the experiments which he made at Winchester with fuming nitrous acid, and what was the result of those experiments? That they prevented the spreading or further communication of contagion*, for that effect is expressly ascribed in p. 193 to the vapour arising from the yellow or smoking nitrous acid, as applied by Dr. Smyth in hospitals and private practice for sixteen or seventeen years before 1795. Whether the facts be true or not, I must beg leave to remark, that, according to his own sense of the phrase, this use of nitrous vapour remained a *dead letter*, for those sixteen or seventeen years.

In the passage now under consideration, there

* The fuming nitrous acid absorbs the respirable portion of the air, when persons are present: it must be therefore impracticable to apply it, in an efficacious quantity to destroy contagion.

seems

seems to be a remarkable want of precision and clearness in Dr. S.'s statement. His words are these: "I formerly mentioned, that
 "I had employed the nitrous acid in two
 "different forms; either the vapour arising
 "from the yellow or smoaking nitrous acid,
 "*which is a mixture of the acid with nitrous gas,*
 "or the more pure nitrous acid detached from
 "nitre, decomposed by the vitriolic acid. In
 "one or other of those forms I have used it,
 "both in hospitals and in private practice, for
 "sixteen or seventeen years past; and have
 "had the satisfaction to obtain the most de-
 "cisive evidence of its happy effect, in pre-
 "venting the spreading or further communi-
 "cation of contagion." Smyth on Gaol Dis-
 temper, &c. p. 193.

If Dr. Smyth had said, I had used it in both these forms, there would have been no room for the imputation of ambiguity, but when he says, I have used it "in one or the other," I may, without impropriety, ask, in which of the two?

In

In what year did he begin to use the nitric acid gas? If he had used it in hospitals and private practice for sixteen or seventeen years, how shall we account for his not using it at Winchester? For surely, if he had used it there, he would have told us so, and he had an opportunity of telling us so pertinently at p. 194, when he was talking both of hospitals and his private practice.

Again, if his experiments of the use of nitric acid gas among his patients had been very frequent, and had continued for a course of years, where was the necessity for transferring his experiments from his own species to a bird and a mouse? Surely the respirability of nitric acid gas would be more clearly ascertained by the feelings of his patients, than of his mouse or his greenfinch. I observe that no date is assigned to the experiments made with Mr. Hume, and I am aware that many *convenient purposes* might be answered by the omission of that date. It must however be presumed, that

that the mouse and the greenfinch were not exposed to experiments, till Dr. Smyth had met with opportunities of trying nitric acid gas upon his patients. My question is, did he avail himself of those opportunities? Did he then know enough of the nitric acid gas to try it upon his patients? Did the result of the trial furnish decisive evidence of its happy effect in preventing the spreading or further communication of contagion? If both analogy and experience had proved this power of the nitric acid gas, before the experiments made with Mr. Hume, there seems to have been little occasion for calling in the aid of two poor little animals.

I am endeavouring to trace Dr. Smyth's intentions and proceedings by the guidance of his printed words. He does not say that he and Mr. Hume made their experiments with any view to ascertain the respirability of nitric acid gas. No, but he does say, that he made these experiments to ascertain with more precision, what were the effects of nitrous acid in fumigation;

fumigation; that very acid which he had frequently used in hospital wards, and in the private apartments of the sick. True it is, the suffocating properties of the fuming nitrous acid may justify the caution of Dr. Smyth; but, if he had used the nitric acid gas as my father used the muriatic acid gas, a very little experience, joined with a very little sagacity, would have pointed out the safe use of it.

During the progress of these experiments the nitric acid gas was perhaps discovered, or at least it was remembered. For now we find it clearly and intelligibly stated, first that the mouse, and then the greenfinch, and then that Mr. Hume and Dr. Smyth themselves were exposed to it. The poor bird was also exposed to the marine acid, and though now and then somewhat uneasy, upon the removal of the jar, it hopped about as lively as before. I wish Dr. Smyth would explain perspicuously and ingenuously, what suggested to his mind trials either of the nitric or muriatic acid gas, during
a process

a process which is professedly instituted for the purpose of some experiments on the *fuming* nitrous acid. How did the powers of the muriatic or the nitric fall into the train of Dr. Smyth's deliberate reflections? The principle of the muriatic had been published by my father in 1758, and by my brother in 1779, and by Morveau in 1773 and 1780. That principle, if known from either of these publications, might suggest to any inquisitive or intelligent man experiments on the nitric, and the idea of this transfer would have occurred the more readily if Dr. Smyth had chanced to read Dr. Priestley's *Observations on Air*, published 1774. "It will be, perhaps, thought
 "that the most useful if not the most remarkable of all the properties of this extraordinary kind of air (nitrous air) is its power
 "of preserving animal substances from putrefaction, and of restoring those that are already putrid, which it possesses in a far
 "greater degree, than fixed air. My first ob-

E.c

"servation

“fervation of this, was altogether casual,” &c.
 Priestley’s Experiments and Observations on
 different Kinds of Air, Vol. I. p. 123.

If Dr. Smyth had been less thrifty in his expenditure of dates, we might have been able to ascertain with more precision, the time at which, and the causes by which, his attention was turned either to the muriatic or the nitric acid gas, and his choice determined to the nitric in preference to the muriatic. Perhaps, however, some rays of light may be thrown on the subject by the manner in which Dr. Smyth announces certain other discoveries of his, and by contrasting his expressions with certain expressions of my father. They are contained in the two books so often quoted in the foregoing pages, viz. :—Dr. Johnstone’s Dissertation concerning the Malignant Epidemical Fever of 1756, printed in 1758; and Dr. Smyth’s Description of the Jail Distemper as it appeared among the Spanish Prisoners at Winchester in 1780, printed in 1795.

DR. JOHNSTONE'S *Dissertation concerning the Malignant Epidemical Fever of 1756, printed 1758.*

DR. SMYTH'S *Description of the Jail Distemper as it appeared among the Spanish Prisoners at Winchester in 1780, printed 1795.*

"Pursuing these views after the emetic had finished its operation, I always ordered the patient to use some antiseptic sedative: spiritus nitri dulcis taken to the quantity of a teaspoonful in mint tea or mint water every three or four hours, will answer this purpose better than any of the neutral saline and emetic mixtures which are generally ordered, and which I also tried, though with less success than followed from the dulcified acids. All those liquid sulphurs, for such in fact are the dulcified spirits of nitre, vitriol, and sea salt, and the anodyne mineral liquor of Hoffman, are certainly excellent remedies in malignant fevers of this class: they allay

"This, however, is greatly obviated by the spiritus ætheris vitriolici, the other medicine which, from a long experience of its efficacy, I have ventured to recommend in the cure of contagious fevers. This medicine has an advantage over most cordials, as it does not increase the heat of the body or the quickness of the pulse, but on the contrary renders the action of the heart more regular and flow."

P. 113.

"This medicine, though long kept in the shops, has very seldom, I believe, been employed in practice; a preparation extremely analogous to it, well known by the pompous title of
liquor

that vomiting, which is for the most part increased by all other medicines, and correct putrefaction both in the stomach and duodenum, and also counteract the influence of the malignant virus in the vascular system, by gently incrassating and preserving the texture of the blood globules. Sweet spirit of nitre in particular promotes very effectually the secretion of urine, and that of perspiration. In these respects they are greatly preferable to the single uncompounded mineral acid spirits. In a word these dulcified acids possess the excellences of the most celebrated alexipharmics, without their pernicious effects and qualities, and that in a very compendious and agreeable form." P. 41, 42.

"In the beginning of the fever the spirits of nitre frequently taken by tea-spoonfuls in mint tea or the patient's common drinks,

liquor anodynus mineralis Hoffmanni being commonly preferred as an antispasmodic; but neither the one nor the other (so far as I have been able to learn) had, in this country, ever been used in the cure of fevers, previous to the year 1769, unless the dulcified spirit of vitriol may be said to have been so, as forming part of the composition of a quack medicine sold in Town by the name of Clutton's Febrifuge Tincture. It was in the summer of the year 1768 that I began to make trial of the dulcified spirit of vitriol in the cure of fevers. From reflecting on the sensible qualities and composition of this medicine I was led to imagine that I should find it useful as a cordial and antiseptic in cases of the putrid and malignant kind. A little experience convinced me that my conjecture was well founded." Appendix, p. 139.

"Tha

drinks, will answer every purpose, and may alone be depended upon." P. 43.

"The necessity of changing the air in a sick room, by successive ventilation, arises from the constant destruction of a certain property in that fluid by breathing, which renders it afterwards useless; likewise from the atmosphere being filled with the excrementitious steams which fly off from the patient's body continually, and which putrify in a stagnant unrenewed air, and render it truly poisonous, a pabulum morbi, rather than of life. The physician will order the room to be kept sweet and clean, and stools and every thing offensive to be removed as soon as possible. If the external air is immoderately cold and wet, the room must be kept warm and dry, and the fumes of amber, myrrh, benzoin, and camphire,

may

"The only merit I can claim, if it can be reckoned such, is the having been the first to point out the efficacy of this medicine in contagious fevers, and I am still the only one who has attended to its effect in reducing the frequency of the pulse." P. 166, 167.

"My attention having been called to this subject, and my character involved in the success, I was satisfied, after the most deliberate reflection on the nature of putrid contagion, that nothing could so certainly or efficaciously destroy it as the mineral acids in a state of vapour, but how to employ these with safety was the difficulty, *hoc opus, hic labor*. The fumes of sulphur which are so successfully made use of to fumigate clothes or furniture could not be employed either in hospital or prison wards, as these fumes are well

known

may be diffused in the room, if sprinkled upon hot iron. Vinegar may be sprinkled about cold, if the weather is warm, and boiled with myrrh or camphire, an antiseptic steam will arise into the air, which the patient breathes greatly to his advantage. These steams will preserve the air free from putrefaction, and will insinuate themselves by the absorbent vessels of the lungs into the blood-vessels, and will greatly assist in impeding the progress of putrefaction in the fluids. These are the most commodious, if not the most useful, methods of medicating the air the patient breathes; however, those who prefer the mineral acids may order brimstone to be burnt, or may raise the marine acid very easily, by putting a certain quantity of common salt into a vessel kept heated, upon a chafing-dish of coals; if to this a small quantity known in small quantity to prove immediately destructive to animal life. *I had frequently remarked* that the fumes of the nitrous acid did not affect the breathing in the same manner; notwithstanding which I conceived they might prove of equal efficacy in destroying or diminishing the virulence of contagion; and *the success attending the employment of these at Winchester*, as well as many trials which I have made both in private and in the Middlesex hospital, have convinced me of the power of the nitrous acid in destroying contagion, and likewise of the safety with which it may be used." P. 54, 55.

"After removing all the hammocks, bedding, &c. from the wards, they are first thoroughly cleaned out, then the hammock posts were well washed with diluted marine acid, and the same thrown,

quantity of oil of vitriol is, from time to time, added, the air will be filled with a thick white acid steam; but both the marine and sulphureous acids must be disengaged at a considerable distance from the patient, otherwise their extreme pungency will be offensive to the lungs." P. 50, 51. So writes my father in 1758 upon his own practice in 1756, an interval of two years.

thrown, by means of garden watering machines, to the upper parts of the posts, as high as the ceiling. The wards when dry were closely shut up, and pots placed in them at different distances, containing from half a pound to a pound of nitre, which was deflagrated by an iron heater put into each pot. The wards were then shut up for some hours, and when opened were exposed to a free ventilation.

After this process had been once or twice repeated the wards were again furnished with fresh hammocks, palliasses, and bedding, instead of the old bedding, &c. which was entirely taken away."

P. 58, 59.

"The floors of the wards (after being swept) must be watered by means of a gardener's watering-pot, with diluted marine acid. The wards are then to be fumigated

fumigated with censers of burning saltpetre, and two or three gallipots about two-thirds filled with fuming spirit of nitre are to be placed in the middle of each ward, to remain constantly night and day, and to be renewed every morning. The beds themselves are to be sprinkled with vinegar."

There are other resemblances of opinion which it is not necessary to adduce. For instance: Dr. Johnstone theorises on putridity, as the cause of malignant fever. Dr. Smyth endeavours to prove that the contagion of jail-fever is "a vapour produced by putrefaction." But is it not singular that Dr. Smyth should conceive himself a discoverer exactly in those points in which Dr. Johnstone has so obviously anticipated him? Is it not very singular that the only two peculiarities of Dr. Smyth's practice should, in their principle, be the peculiarities

rities of my father's? It is clear that my father recommended the dulcified acids in 1758, and that he used them in 1756, and yet such is Dr. Smyth's love of discovery, such his ardour for originality, that he claims this practice as peculiarly his own. It is singular too that both Dr. Smyth's discoveries are ushered in with the same phraseology, "From reflecting
 " on the sensible qualities and composition of
 " this medicine (*spiritus ætheris vitriolici*) I
 " was led to imagine that I should find it use-
 " ful as a cordial and antiseptic in cases of the
 " putrid and malignant kind."

When Dr. Smith discovers the powers of mineral acid vapours, *this* too is the work of deliberate reflection. "I was satisfied after
 " the most deliberate reflection on the nature
 " of putrid contagion that nothing could so
 " certainly or efficaciously destroy it as the mi-
 " neral acids in a state of vapour." How unfortunate is it that so much deep reflection

F f

should

should have been wasted in discovering in 1780, that which had been promulgated in 1758, in 1773, and in 1779! For at the first of these periods my father, without any parade of novelty, any pomp of diction, any professed felicity of conjecture, any boasts of deep and deliberate reflection, had declared the use of the dulcified acids as medicines best suited to counteract the consequences of contagion when operating upon the living body, and that the mineral acids in a state of vapour were useful means of counteracting putrefaction and contagion out of the body. I must again lament this waste of genius; for had the same powers of reflection been employed on some other subject there is a possibility that one or other of those diseases which now embitter the comforts of life and shorten its duration, would have been expunged from the mournful catalogue of human woes.—But to proceed: After reading with surprise and some distrust, the imaginations,

imaginations, conjectures, repeated trials, and
 deliberate reflections of Dr. Smyth, I was sud-
 denly and agreeably refreshed by the new dis-
 play of his talents for confession. "It will
 " probably be asked why I did not make a
 " complete trial of it (nitrous acid) there (at
 " Winchester). To this I answer, that with re-
 " spect to fumigating infected clothes, bed-
 " ding, &c. I did not think myself warranted,
 " especially on an occasion of so much im-
 " portance, to make trial of *an uncertain re-*
 " *medy when a certain one was in my power.*
 " As to fumigating the prison and hospital
 " wards, it evidently was *my intention* to have
 " employed the nitrous acid, but I was mis-
 " taken in the means I took to procure it, and
 " *have not since had a proper opportunity* of re-
 " peating the experiment." P. 195.

The reader will probably recollect that Dr.
 Smyth's book does mention that gallipots of
 fuming nitrous acid were employed in 1780

at Winchester. He will also find that Dr. Smyth's bad health, and the peace which followed in 1783, are assigned by him as reasons for laying aside the publication of what he had done and observed at Winchester till he was led to revise his notes, in consequence of an application from Dr. Morris, in what he calls "last summer," by which I understand 1794, as he dates his dedication 14th August, 1795. During this interval the Doctor, I suppose, shared with other men of science the great improvements which had been made in chemistry, and which would probably instruct him that the combustion of common nitre (nitrate of potash) did not exactly furnish nitric acid gas, or air purer than that of the atmosphere. Guided then by the discoveries of chemists in their own science, and knowing, as he must, the inefficacy of small and the dangerous nature of great quantities of *summing* nitrous acid, he would probably learn that he

had

had *not* employed "the nitrous acid in two different forms,"* that he was mistaken in supposing that deflagrated nitre gave out nitric acid gas, and that had the patients at Winchester inspired his *fuming* nitrous acid some of the effects of suffocation would have ensued. I am still perplexed to ascertain the time at which either the application or the discovery of the nitric acid gas is to be fixed, and the embarrassment which I feel from the ambiguity of Dr. Smyth's language, is only partially relieved by his confession, that he did not make a complete trial of the nitrous acid at Winchester. Yet he lays claim to the merit of *intending* to have employed the nitrous acid, but he confesses that he was mistaken in the means he took to procure it, and adds that he has not since had a proper opportunity of repeating the experiment.

This error, and this failure in procuring the

* Smyth, p. 193.

nitrous acid, constitutes a complete proof that he did not use the nitric acid gas at Winchester, that his deliberate reflections on the power of mineral acids had not been very frequently accompanied by previous trials upon his patients, and that the result of those trials had produced suspicion without conviction, deliberation without decision, and intention without action, whilst he was performing the high functions of his mission to Winchester.

Thus then, at p. 55 of his book, Dr. Smyth talks of his successful use of nitrous acid at Winchester.* At p. 25 of his Letter he says,

* My readers must have observed that in my quotations from Dr. Smyth's Letter I sometimes have laid before them his meaning embodied in complete paragraphs, and then have broken them up into parts, where "*disjecti membra sophistæ*" seem to me endowed with those properties of artifice and annoyance, which characterize both the statement and the reasoning of my opponent. The necessity for doing so was imposed upon me partly by the intense watchfulness and the fierce indignation of Dr. Smyth on the subject of quotation, and partly by the peculiar talent which he possesses for slurring over that which I wish

to

“ the three mineral acids were employed by
 “ him at Winchester, and his opinion of their
 “ superior efficacy for destroying contagion
 “ was communicated by letter to the Board of
 “ Sick and Hurt, and mentioned publicly by
 “ me on many occasions.” At p. 195 of his
 book he declares, “ that he was mistaken in
 “ the means of procuring it, but that he *disco-*
 “ *vered an intention* to use it.” What inference
 must we not draw from all this palpable con-
 tradiction but that he did not use the respirable
 mineral acid gases as antiloimics at Winchef-
 ter, that he did not use them till he wrote his
 book, not having had proper opportunities of
 repeating the experiment? What experiment?

to be distinctly examined, and for confounding what I have
 often been compelled to separate. This apology will, I hope,
 justify the occasional repetition of the same passages from Dr.
 Smyth's book, my father's, my brother's, and my own. *Acce-*
dere oportet ad singula, sic universa franguntur.

CIC. *Oratorie Partitiones.*

Why,

Why, that very experiment which he has the instant before confessed he had failed in! When he does make experiments, he exposes a mouse and a greenfinch to the mineral acid vapours, to ascertain whether they are respirable, forgetting his former declaration, that in his private practice, and at the Middlesex hospital, he had frequently remarked that nitrous vapour did not much affect respiration. Such was the result of the *hoc opus, hic labor*, of Dr. Smyth; and truly upon few occasions could the adage be better applied, *Parturiunt montes, nascetur ridiculus mus*.

Before I finish this topic let me remark further, that although Dr. Johnstone and Dr. Smyth use the same principle of practice in the two instances before adduced, yet the agents are different. In every thing Dr. Smyth is resolved to be original. My father recommends all the dulcified acids upon a like principle, but adheres chiefly to the *spiritus ætheris nitrosi*.

nitrosi. Dr. Smyth recommends exclusively the *spiritus ætheris vitriolici*, not forgetting, however, “a preparation extremely analogous to it, well known by the pompous title of “*liquor anodynus mineralis Hoffmanni*.” When Dr. Smyth, after deliberate reflection, uses mineral acids in a state of vapour, he does not select that acid which has been tried before the muriatic, but his mind undergoes all the agonies of intellectual parturition to bring forward the nitric.* *Hoc opus, hic labor*. His first effort does not succeed: in this throe he only brings forth an INTENTION. In his second, fourteen years afterwards, he is more fortunate, and all his past pangs may be supposed to be

* If the reader be a partisan of the Darwinian Theory of Generation, it will perhaps strike him as possible, that whilst in the heat and agitation of that deliberate reflection which produced the fuming nitrous acid, Dr. Smyth might have had his mind's eye intently fixed on certain charms and qualities of muriatic acid gas.

forgotten in the education of a promising brat, who, in less than fourteen years more, is to be richly endowed. The muriatic he treats as a foundling, whom accident had thrown in his way, and though in some of its features it bore a pretty strong resemblance to his own favourite offspring, yet after a little inspection, he rejects it, as an unnecessary incumbrance to himself, and leaves to be picked up by its own parents, or by some generous protector, who might have more leisure or more inclination to nourish and bring to light any good qualities with which it might be endowed.

In respect then to the muriatic acid gas, to the fuming nitrous acid, and to the dulcified acids, I make the following observations. The use of the one by my father was antecedent to the use of the other by Dr. Smyth. The publication of that practice by my father was previous to Dr. Smyth's practice. The resemblance in the principles, in both cases, is complete.

plete. The variation of the agents, as announced by Dr. Smyth, is very remarkable.

Now this double coincidence of identity in the principle, and diversity in the instruments, though in the case of Dr. Smyth it might be the result of mere accident, still must be allowed to carry with it the ordinary appearances of deep contrivance and dextrous execution—of a mind conscious that something had been previously discovered, and that something else was to be added or altered to support pretensions to a first and pure discovery, which, in reality, was only a second and qualified one. I do not, however, intend to press any invidious considerations of this kind. I am aware that the *just* claims of Dr. Smyth are compatible with those of my father in every circumstance except that of *priority*; and therefore I virtually maintained in my statement, as I have repeatedly affirmed in this Reply, that Dr. Smyth is not the sole, nor even the ori-

ginal discoverer, employer, and promulgator of the principle, for the discovery, employment, and promulgation of which he was some time ago a claimant to Parliament, and has since stood forth a competitor with my father.

I wrote my pamphlet after deliberate reflection. Dr. Smyth, doubtless, prepared his book on jail fever, and his Letter to Mr. Wilberforce, after deliberate reflection. For the sake of precision in my statements, clearness in my reasoning, and justice to my father's cause, I have again and again endeavoured to separate the points really in dispute between Dr. Smyth and myself, from collateral considerations, and I respectfully entrust the decision, to the deliberate reflections of impartial and intelligent readers.

In my pamphlet, "On the Discovery of the
 "Power of Mineral Acids in a State of Va-
 "pour to destroy Contagion," I have related
 my

my father's method of raising muriatic acid gas, and Dr. Smyth's method of raising nitric acid gas. I have also related some striking instances of the power of muriatic acid gas in destroying contagion, and some experiments on the relative convenience of application of the muriatic and nitric acid gases, by some learned friends. "On their observations (the observations of "my friends, says Dr. Smyth,) I shall only remark that they come too late and prove too "much."* YES: they would have come too late, for a man who is intent only on emolument, and they do prove too much, for a man who would darken the truth.

On the assumed superiority of the nitric acid gas over the muriatic, in regard to the ease and safety with which it may be breathed, I shall now say a few words. The committee of the House of Commons, to whom Dr. J. C. Smyth's petition was referred, "pointed out

* Letter, p. 28.

"the

“ the fundamental and important distinction
 “ between the nitrous and other two mineral
 “ acids, (to be) that the former can alone be
 “ respired without injury, and therefore can
 “ alone be used, except in places, from which
 “ the patients and all other persons have been
 “ removed,”* and upon this distinction pro-
 claimed Dr. Smyth the discoverer of the prin-
 ciple, in its only practicable shape, and recom-
 mended him therefore to be rewarded.

Now it is remarkable that Mr. Davy, who
 gave evidence before the committee, told them,
 “ he believes the fumes of the marine acid
 “ equally diluted are respirable with equal
 “ safety.” To Mr. Davy’s chemical attain-
 ments I do pay unfeigned homage, but he will
 pardon me for imagining, that the difference
 of sensation which he mentioned afterwards,
 arose from the muriatic acid gas being breathed
 by him, in a less diluted state than the nitric.

* Report, p. 7.

Dr. Andrew Duncan, in the Annals of Medicine for 1803-4, closes a long account of the report on Dr. Smyth's petition with the following observations: "But during the course of February, March, and April, 1804, Dr. Duncan founding on the observations of Guyton Morveau, employed in the clinical wards of the Royal Infirmary of Edinburgh, common salt in place of nitre with no less advantage, and he thinks with less inconvenience." P. 463. That muriatic acid gas is, to a certain degree, respirable, Dr. Smyth admits,* thus contradicting the fundamental position of the committee on his own case. That it can be respired undiluted, I never have asserted: but that it can be respired with equal utility, safety, and convenience, in like quantities with the nitric, I shall now prove; and also that it could not be the inconvenience or danger attending its use.

* Letter, p. 28.

that

that caused it to be discontinued at Edinburgh, and never used, as Dr. Smyth says, "so far as I know *any where else." I presume that this is an error of memory, or of the press, for in two pages afterwards Dr. Smyth, speaking of his own book, says: "This Treatise contained also a relation of the trials made with the marine acid gas in the hospitals of France."† Now the year 1794 was certainly subsequent to the year 1779, and trials made in the hospitals of France were certainly made somewhere. But if trials of muriatic acid gas were made *any where* with effect and safety, then are the committee of the House of Commons mistaken in their fundamental position. They were repeatedly made both in France and England before the committee were called upon to decide, and it cannot but be esteemed unfortunate, both for the cause of science as well as the cause of justice, either

* Letter, p. 24.

† Letter, p. 26.

that

that these trials were not seen in their true light, or were not observed at all. By dedicating my pamphlet to the House of Commons, I did mean to appeal to the judgment of the House from the judgment of the committee, and with all due respect to the authority of both, I do now confess myself not satisfied with the decision of either. But I shall not again return to the charge.*

Instead of explaining what value is to be set upon the testimonies which I have produced from my friends, or upon the experiments and calculations, which I made for ascertaining the equal if not the superior convenience in application of the muriatic acid gas to the nitric, Dr. Smyth contents himself with declaring, that "the preceding authorities *is*† the only " answer he shall give to the experiments of

* Smyth's Letter, p. 28.

† The word "*is*," thus decorated with italics, is copied from Dr. Smyth's Letter exactly.

“ my friends.” “ Forgetful that a relation of
 “ experiments and observations, by persons qua-
 “ lified to make them, of undoubted veracity,
 “ and free from all suspicion of partiality or
 “ prejudice, is the only evidence that a subject
 “ of natural knowledge can admit.” Smyth
 adversus Trotter, Appendix to the Report, No.
 35, p. 92.

I shall therefore again bring forward those testimonies, shall again insist upon the propriety and validity of my own statement; I shall add to them the result of further experience, and by this process I shall hope to prove, that the authorities employed by Dr. Smyth are not quite so decisive as he may suppose them to be.

That muriatic acid gas is a power capable of destroying contagion, the committee of the House of Commons did not deny. I shall beg the attention of the reader to the following striking evidence of its efficacy, which is copied

"pied from my pamphlet: " Four years ago
 " Mr. Bloxham, surgeon and apothecary at
 " Halefowen, asked my advice concerning a
 " malignant fever which had proved very fatal
 " to many persons, and had continued long in
 " the poor-house of that town. For as often
 " as fresh paupers were brought into the house
 " they received the infection, though it had
 " apparently spent its violence upon those who
 " had resided in it for some time. The house
 " had been white-washed, and the ordinary
 " precautions of washing the floors with vine-
 " gar, burning tar, &c. were used in vain. I
 " advised him to fumigate all the apartments
 " with the muriatic vapour, and to continue it
 " for many days. This was done thoroughly,
 " and afterwards there was no instance of the
 " recurrence of contagion."

" I could fill a volume with cases in my own
 " practice in which muriatic vapour was effica-
 " cious in preventing and destroying contagion.

“ The following will undeniably prove that
 “ this vapour can be used in very large quantities, in places from which persons have not
 “ been removed :

“ In Mr. Wilson’s house, consisting of two
 “ rooms, about fourteen by twelve, and a small
 “ staircase between, on each floor, the father
 “ and four children had scarlet fever and ulcerated sore throat. Half a pound of salt was
 “ placed in every apartment except the kitchen, and upon the two landings of the staircase, the whole moistened in the course of
 “ two days with lb. iss. of vitriolic acid ; that is,
 “ more than three ounces of acid to each pound
 “ of salt, making in all seven vessels yielding
 “ vapour. In this small space the smell of
 “ muriatic acid was very pungent during two
 “ days, and yet after the first mixing, it was
 “ respired not only without difficulty, but even
 “ with pleasure as well as advantage.

“ Miss Greaves had dreadful putrid fever :

“ she

“ she lay in an apartment at most nine feet
 “ square, without any chimney. On first mix-
 “ ing the vitriolic acid and salt (as is very com-
 “ mon) the ingredients were mixed together so
 “ incautiously and in such large quantity that
 “ the room was instantly overloaded with va-
 “ pour, and cough excited. But this inconve-
 “ nience was remedied by after-experience, and
 “ the room kept full of vapour for ten days.
 “ She recovered, and not one of a large family;
 “ and of the attenders in a public shop, was
 “ infected. Here then is a proof that respiration
 “ can continue in a room nine feet square
 “ filled with muriatic vapour, not only with-
 “ out inconvenience but probably a public
 “ blessing, by preventing the diffusion of a ma-
 “ lignant contagion in the centre of a popula-
 “ tion of seventy-thousand souls.

“ Mr. Harris, jun. had ulcerated fore throat,
 “ with eruptions all over the skin, but espe-
 “ cially of the thighs, strikingly like the measles,
 “ and indeed at first mistaken for that disorder
 “ till

“ till some of them terminated in black and
 “ purple spots. In this alarming case the mu-
 “ riatic vapour was recommended and perfe-
 “ vered in for many days. The young man
 “ recovered; and none of a large family, who
 “ had free access to the sick apartment, were
 “ infected. In mixing up the ingredients for
 “ the first time, the nurse held the vessel con-
 “ taining salt in her hand, and poured the acid
 “ upon it, just under her nose; of course it
 “ excited coughing. So soon as it was diffused
 “ over the room it had no such effect, but
 “ was exceedingly grateful to the patient.

“ In the use of muriatic vapour this caution
 “ must always be observed: On first mixing
 “ the acid with the salt, the vapour imme-
 “ diately arises in larger quantity than it does
 “ from nitre, and the undiluted rising steam
 “ of vapour should be avoided till diffused ge-
 “ nerally over the apartment. All costly furni-
 “ ture, polished iron, gilt chairs, coloured li-
 “ nens, should be removed, as the muriatic
 “ acid

“ acid gas corrodes the metallic bodies, and
 “ discharges the colours of the linens and
 “ weakens their texture. The nitric vapour
 “ has the same effect, but it rises more slowly,
 “ and in the proportions ordered by Dr. Smyth
 “ there hardly appears any fume, and the smell
 “ is not very perceptible. It should be re-
 “ marked likewise, that from equal weights
 “ of nitre and common salt, decomposed by vi-
 “ triolic acid, nearly one-fourth more gas is
 “ extricated from the common salt than from
 “ the nitre ; as one hundred parts of common
 “ salt contain nearly fifty parts of muriatic acid,
 “ whereas one hundred parts of nitre only con-
 “ tain thirty-four or thirty-five of nitric acid.

“ On the comparative convenience or incon-
 “ venience of application of the nitric and mu-
 “ riatic vapours, I shall now give the decisive
 “ evidence of experiments made in the infir-
 “ mary of Worcester and the general hospital
 “ near Birmingham ; and I shall subjoin the
 “ testimony of an intelligent and amiable lady,

“ to

“ to whom the afflictions of a large family have
 “ unfortunately afforded too much scope for
 “ accurate observation on this subject. The fol-
 “ lowing is the result of the experiments made
 “ by my learned friend Doctor Skey, physician
 “ to the Worcester infirmary.

“ DEAR SIR,

“ I regret that various circumstances have
 “ combined to prevent my returning you an
 “ earlier answer to your letter. The time,
 “ however, which has elapsed has enabled me
 “ to give a fairer trial of the two acid vapours,
 “ as to their respective effects on the lungs.

“ The general result of the experiments
 “ made in three of our wards, namely, two
 “ on the women's side and one on the
 “ men's, with as accurate an attention as I
 “ could pay to ascertain the fact in similar
 “ circumstances, was, that on the whole
 “ more distress was felt by the patients when
 “ the ward was filled with nitrous acid va-
 “ pours than when with muriatic. The first
 “ gave

gave rise to sickness, vomiting, cough, and dizziness, the last did so too, but in a much inferior degree; such at least was the almost unanimous opinion of the patients themselves. I ought to remark, that as the object was to try the relative effects of the acid vapours on breathing, in both cases the patients were more exposed to the vapours than is commonly necessary in fumigating. To this must be attributed the more than usually disagreeable effects of the nitrous acid vapour, which occurred particularly in one of the women's wards, where were several much debilitated patients.

I remain, dear sir,

Yours faithfully,

(Copy.)

Jos. SKEY.

Worcester, Sept. 22, 1802.

In the general hospital near Birmingham the following experiments were made under the inspection and direction of the Rev. George

I i

Hickes,

Hickes, A. M. rector of Burnfall, in Yorkshire, and of Mr. Partridge, house surgeon and apothecary to the hospital.

“ In two unoccupied lower wards, each
 “ about thirty by twenty-one, and fifteen feet
 “ high, were placed two earthen vessels, with
 “ a pound of sulphuric acid in each. In the
 “ vessel on one side a pound of common salt
 “ was added to the acid, and in the other a
 “ pound of common nitre, and both rooms were
 “ shut up for some time. The ward with the
 “ mixture of common salt and sulphuric acid
 “ was full of vapour, which was pungent to the
 “ nose but excited no cough, and was thought
 “ pleasant. A water-closet adjoining, which
 “ before was very offensive, in two or three
 “ minutes was rendered perfectly sweet on the
 “ door being opened and muriatic acid gas ad-
 “ mitted into it. In the corresponding ward
 “ the smell of nitrous vapour was not near so
 “ strong, nor when we stood over the vessel
 “ did

“ did it so sensibly affect the nostrils as the
 “ muriatic; but it much more strongly irri-
 “ tated the top of the windpipe, producing
 “ the irresistible necessity of coughing; and the
 “ gentlemen observed that the chief differ-
 “ ence of their feelings in the two wards was,
 “ that in the one the muriatic acid gas was more
 “ sensibly felt in the nose, affecting it with a
 “ sort of tickling, which was far from unplea-
 “ sant, and that in the other the nitric acid
 “ gas always affected the throat so strongly
 “ as necessarily to induce coughing, (which
 “ was not the usual effect of the muriatic) but
 “ did not irritate the nose.

“ In the two large middle wards three ves-
 “ sels, each containing half an ounce of sul-
 “ phuric acid, were placed, according to Dr.
 “ Smyth's direction, at the distance of fifteen
 “ feet from one another, and an equal weight
 “ of nitric was added to the three vessels in
 “ one ward, and of salt to the same number
 “ in the other. To the vessels containing
 “ nitre heat was applied. The smell of the

“ two acid vapours was very faintly percep-
 “ tible in both wards, and the patients felt
 “ no inconvenience from, and expressed no
 “ dislike of, either.

“ Mr. Partridge, who has repeatedly used
 “ both the nitric and muriatic vapours in the
 “ different wards of the hospital, upon the
 “ whole finds the muriatic more agreeable to
 “ the patients than the nitric, after it has been
 “ diffused over the apartment. On its first
 “ rising, and till it was diffused, it of course
 “ could not be agreeable. The difference,
 “ however, he thinks inconsiderable, and he
 “ finds both equally useful for destroying the of-
 “ fensive smell of crowded wards, and both
 “ nearly equal in convenience of application.”

My friend Mrs. Whateley's letter is decisive
 of the fact of convenience ; and on this point I
 would prefer the experience of a tender mo-
 ther, watching over her sick child, to the opi-
 nions of all the chemists in Europe. It is a
 plain question of fact, cognisable by the senses,
 and ascertainable by the simplest and most easy
 experiments.

Birmingham, December

DEAR SIR,

14th, 1802.

Having unfortunately had occasion, from the illness of my children, to make use of fumigation, and hearing of your intention to publish on the subject, I send you the following statement: My youngest boy, a child eight years of age, was very dangerously ill in November last of a malignant fever and putrid sore throat. By the advice of our surgeon, Mr. Freer, I fumigated the room with the muriatic acid vapour, and though a young servant girl and myself constantly attended him, and we had also continual intercourse with the rest of the family consisting of nine other persons, not one had the slightest attack of the disease. I fumigated the room so strongly that a white cloudy vapour was visible constantly floating in it, yet I perceived not the smallest difficulty of breathing; and the child, though the vessel which contained the fumigation stood very near his bed, appeared and expressed himself refreshed

refreshed by it; and an old nurse more than seventy, and asthmatic, perceived not the least inconvenience; on the contrary she thought her cough allayed by it.

On a former melancholy occasion I used the nitric acid vapour, which I found troublesome from the heating of the sand; and on approaching too near the vessel which contained this fumigation and pouring in more vitriol I was nearly suffocated, and with difficulty got out of the room, and the unpleasant effects did not leave my lungs of more than two hours. I approached the vessel which contained the muriatic fumigation as near frequently, and renewed the process (of pouring the vitriol on common salt) but was affected in an infinitely slighter degree.

I am, dear sir,

Yours truly,

(Signed)

MARY WHATELEY.

At this period, whilst I am writing this part of my reply to Dr. Smyth, I daily attend a female

male servant of my estimable friend Mr. J. Crompton. She has malignant fever of the worst kind, and the smell of the room when the muriatic acid gas rose in diminished quantity, has been once or twice intolerably offensive. But the offensive smell has been always removed by renewing the process, and raising the gas. In a room seventeen feet square two pounds of sulphuric acid, with an adequate proportion of salt, have been used every twenty-four hours for five days past, without any inconvenient pungency. To-day (April 6th) a deep jug, filled with two or three pounds of salt, constantly stirred up and supplied with sulphuric acid, is used. The muriatic acid gas is very perceptible, not at all disagreeably pungent, and the room is perfectly sweet, although the case is of the most desperate and putrid kind. In this instance I used a quantity of muriatic acid gas, nearly five times greater than that of nitric acid gas, recommended by Dr.

Smyth

Smyth in the same space, and entirely without inconvenience.*

The whole question of convenience of application indeed appears to me to depend upon quantity. In equal quantity, muriatic acid gas is at least as useable as nitric, as useful too, and probably more agreeable. Both must be diluted with atmospherical air before they can be breathed. The undiluted steam of rising gas of both must be avoided. Mr. Menzies, in "Account of the Experiment made on board the Union Hospital Ship," p. 12, says: "I however proceeded in this first trial slowly and cautiously, following with my eyes the pipkins in every direction, to watch the effect of the vapour on the sick, and observed that at first it excited a good deal of coughing,

* As to the quantity of muriatic acid gas which can be borne without inconvenience in a given space, I find that in an apartment containing 360 cubic feet respiration is not disturbed by the gas extricated from one ounce of salt by one ounce of sulphuric acid.

" but

" but which gradually ceased in proportion as
 " it became more generally diffused through
 " the wards." This fact proves ^{sometimes} that the nitric
 acid gas may excite coughing as well as the
 muriatic, and that *it requires to be disenga-*
ged at a distance from the patient, otherwise
its extreme pungency will be offensive to the
lungs. I will not, however, deduce from
 this experience of Mr. Menzies, as Dr. Smyth
 has done from the caution of my father, that
 it plainly shews " how little he thought them
 " (mineral acid gases) calculated for being em-
 " ployed in the apartments of the sick."* I
 shall not descend to the mean competition of
 extolling the muriatic for the sake of depre-
 ciating the nitric acid gas. They are both use-
 ful upon the same principle, and both may be
 used for the same purposes, with no great differ-
 ence of convenience. What difference there
 is, plainly is in favour of the muriatic, from the
 simplicity of the process by which it can be

* Smyth's Letter, p. 8.

raised. An earthen vessel, filled with salt, and free from all metallic substances, and a bottle of sulphuric acid, are the only instruments necessary in the disengagement of it, to which may be added occasionally water and heat. The only caution necessary, is, not to pour too much sulphuric acid at once upon the salt. Pour it on by little and little, and frequently, and now and then stir up the mixture with a stick. When the atmosphere is very dry, and a quantity of moisture does not adhere to the salt, the application of the agents to each other is aided by the addition of a little water. In general the caloric let loose by the mixture of sulphuric acid and muriate of soda, is adequate to all the purposes of extricating muriatic acid gas. Whenever it is wished that a large quantity of gas should be diffused quickly, then the vessel may be placed on hot coals, or in any other way exposed to heat. For the purpose of correcting the vitiated air of the chamber in which the sick man lies, and of destroying contagious

tagious matter, the simple pouring of sulphuric acid in small portions, and at intervals, on common salt is sufficient. But whilst there is any chance of contagious matter being formed the muriatic acid gas should be raised without intermission; the smell of it in the room should be always perceptible. It will answer little good purpose to use it one hour, and intermit it the next. The poison of contagion, formed by pestilential diseases, is every instant exhaling from the lungs the fauces and the excretions of the body; every instant, therefore, should it be met, and consumed by the muriatic acid gas. Wheresoever it lurks it must be met, and therefore should the bed-clothes be frequently turned down and a current of muriatic acid gas be passed through them. All infections are equally destroyed by the application of the mineral acid gases, if we may rely on the observations of Morveau and many of our own ingenious countrymen.* I direct the

* Crawford, Cruikshank, &c.

muriatic acid gas frequently in cases of infection unattended by fever, when rooms are rendered offensive by venereal ulcers, and other putrid exhalations of animal matter. I do not mean to say that the constitutional effects of these poisons, when acting on the living system, are cured by it, although I believe that recovery may be facilitated and even hastened by the patient being thus placed in circumstances more conducive to his comfort, and more favourable to his general health. The great and important action of mineral acid gases is in decomposing and consuming the poison and contagion of dead animal matter. In no respect do they supersede the necessity of strict attention to cleanliness, and of constant ventilation.

The disadvantages of the application of nitric acid gas in comparison with the muriatic are, that it requires more attention and gives more trouble. Heat must be applied, and consequently the effects of fire must be guarded against; which in ships is a consideration of great

great moment. The materials are more expensive, and after all, the nitrate of potash is seldom bought sufficiently pure; it is generally adulterated with salt. This adulteration may occasion considerable inconvenience by letting loose nitro-muriatic acid gas when sulphuric acid is poured upon the mixed salt, and this acid gas is very suffocating. The nitric acid gas is less expansible than the muriatic, and is sooner condensed. Nitric acid gas only gives out oxygen*, by being converted into nitrous gas,

* It was my father's opinion that in the extrication of nitric acid gas a larger portion of oxygen was communicated to the air. This opinion is not chemically correct; but my father has erred in adopting it, on a point in which that most ingenious and celebrated chemist, Mr. Keir, was content to doubt. "Whether these white fumes derive any advantage from the vital or oxygenous air that is extricated from the mixture of oil of vitriol and nitre, I will not presume to offer an opinion." Report, ap. 37, p. 116.

Guyton Morveau has fully discussed this point, and he asserts that no oxygen is communicated to the air, except by the conversion of nitric acid gas into nitrous gas.

I allow

and nitrous gas acts only on the respirable portion of the air. Finally, if metallic substances are in contact with the mixture, the suffocating irrespirable nitrous gas is let loose.

If then we compare the two processes fairly it appears to me that the preference must be given to the muriatic, which, I again assert, in equal quantity to be equally respirable and agreeable as the nitric acid gas.

The opinions of professor Odier, of M. Mojon, and of Dr. M'Gregor, if we had the power of exactly ascertaining the grounds on which they were formed, I have no doubt would be found to depend upon quantity. Dr. Smyth

I allow with Mr. Keir that the application of nitric acid gas was the proper discovery of Dr. Smyth; I allow him the merit of the further discovery, "which distinguishes it from the red vapour of nitrous acid, and from the fumes of sulphur." But I must except the diluted fumes of salt, which Mr. Keir has not excepted. I am confident if this accurate and sagacious observer will be pleased to make the comparative experiments, that he will find the muriatic acid gas, to be at least equally respirable with the nitric, in like quantity.

recommends

recommends nitric acid gas in such small doses that it can be never disagreeable, often scarcely perceptible when his rule is strictly followed.

The president of the criminal tribunal mentioned by Dr. Odier, must have given his orders solely from quantity. The muriatic acid gas was rising in excess at the moment of his entering the prison; perhaps, the attendants seeing his approach, added on the spur of the occasion, too much sulphuric acid to the salt, and thus exposed him to the pungent effects of an undiluted cloud of gas. Under the same circumstances had nitre been used instead of salt, the effects would have been the same, and he then probably would have ordered the muriatic instead of the nitric fumigation.

Dr. McGregor's patients had an over-dose, otherwise there could have been no difficulty in applying, and continuing to apply, muriatic acid gas whilst the patients were confined to their beds. I again repeat that in all these seeming
exceptions

exceptions the muriatic acid gas was first of all raised in excessive quantity, and probably discontinued without further trial. I cannot otherwise account for the different result of the experiences of these gentlemen from my own, and from the common experience of this country. I cannot otherwise account for the very different result from that of Dr. Duncan, sen.* who in 1802 employed nitrous fumigation with good effect, and who in 1804 employed muriatic with no less advantage, and he thinks *with less inconvenience*.†

The experiments of Guyton Morveau have, however, determined this point; and I shall now adduce some of them, as they fix an everlasting seal upon the discovery of my father.

Traité des Moyens de désinfecter l'Air, de prévenir la Contagion, et d'en arreter les Progres. Par L. B. Guyton Morveau, à Paris 1801.

* Appendix to the Report, No. 12, p. 50.

† Annals of Medicine, 1803-4, p. 463.

“ *All the mineral acids possess the power
 “ of destroying contagious miasmata, and the
 “ smells which announce their presence, espe-
 “ cially when in a state of vapour or gas. But it
 “ may be asked are they all efficacious in the
 “ same manner, are they all equally prompt and
 “ complete in their actions?” To ascertain the
 fact, Morveau made experiments on the gas
 extricated from putrefying meat, and the result,
 as drawn up by Berthollet, proved that

† 1. The gas produced by putrefaction con-
 tains a good deal of carbon and no hydrogen.

2. Urine exposed to light in closed vessels
 remains acid. In the dark it forms am-
 monia.

3. Meat kept fifteen days in closed vessels
 with a small quantity of water, rendered the
 water acid with a little ammonia.

4. This meat, on stewing it, was still ca-
 pable of affording jelly.

* P. 65.

† P. 79.

5. The gas from putrefaction gave him the colic twice. It is necessary to make experiments with it cautiously.

6. The putrid principle in the air is not absorbed by lime water, but when dissolved in water it is.

7. A substance not putrid can absorb a good deal of this gas without putrefying, but at a certain point it is much disposed to putrefy.

8. Water is composed in the greater number of putrefactions, but there is no development of hydrogen.

9. The most antiseptic substances are bark and nut-galls.

By exposing the gas from putrefaction to various powers, Morveau deduces the following facts: “* *Water* does not destroy the odour, on the contrary retains it. *Lime* is only useful for decomposing animal substances before putre-

* *Moyens de Désinfecter l’Air*, p. 270, &c.

“ faction

“faction commences, or for absorbing the car-
 “bonic acid. Fresh flacked lime in large quan-
 “tities suspends for a time the cadaverous ema-
 “nations. Air full of putrid miasmata is not
 “purified by passing through lime water. *Re-*
 “*sinous substances* only mask for a moment the
 “putrid odour, without purifying the air. No
 “advantage is to be expected from *fires*, and
 “*throwing various substances on hot coals*. Vi-
 “negars are burnt rather than converted into
 “vapour; nitre only lets loose unrespirable gas,
 “after having rendered the fire more intense
 “by its oxygen; and firing gunpowder exer-
 “cises only a mechanical effect on the air.
 “*Sulphur* acts otherwise. Thrown on burning
 “coals it gives out a sulphureous vapour, which
 “destroys the putrid miasmata, but does not
 “diffuse itself to any distance, and is unsup-
 “portable in inhabited places. It may be useful
 “for purifying infected merchandize, and re-
 “moving stagnant air. *Common vinegar* may
 “be reckoned one of the best antiloimics for

“ bodies that can be plunged in it, or well
 “ washed by it. Its vapours are not sufficiently
 “ expansible for the purpose of fumigation.
 “ *The acetic acid* is not more expansible than
 “ common vinegar, but its action on infected
 “ substances is more rapid and intense ; it is
 “ a personal preservative not to be neglected
 “ by those who are obliged to expose them-
 “ selves to putrid effluvia.

“ It is generally known that the mineral
 “ acids are antiseptic, that they put a stop to
 “ all vegetable and animal fermentation, and
 “ that they decompose the contagious poisons.
 “ *The sulphuric acid*, on account of its fixity,
 “ cannot be employed for purifying air. Even
 “ in its concentrated state Dr. Crawford ob-
 “ served that it was less prompt in destroying
 “ the fetid odour of animal hepatic gas, than
 “ the nitric and oxygenated muriatic acids.
 “ We have seen how far sulphureous acid
 “ gas could be employed before.

“ The nitrous acid only acts on the respirable
 “ part

“ part of air ; the vapours it exhales are suffo-
 “ cating. The nitric acid disengaged after
 “ the manner of Dr. Smyth destroys entirely
 “ putrid miasmata,* but it rises little, it con-
 “ denses soon, and only acts as an oxygenator
 “ in giving out nitrous gas; it is only by re-
 “ peating the operation frequently, even in a
 “ confined and narrow space, that we can rely
 “ upon its efficacy. Finally, the operation
 “ requires precaution in the choice of mate-
 “ rials, and the manner of using them. I do
 “ not speak of the consumption of nitre, which
 “ ought to be perfectly pure, and will be con-
 “ sequently dear, this would suppose us ca-
 “ pable of trading in the preservation of hu-
 “ man beings. *The muriatic acid*† presents
 “ here the greatest advantages, on account of
 “ its prodigious expansibility, which enables
 “ it to reach presently the matters to be acted

* Exp. xxvi, xxxii, xxviii.

† Exp. xxix, xxxii, xxxiii, xxxiv.

“ upon.

“ upon. The manner of employing it too is
 “ very simple and unexpensive, and exposes
 “ ships and combustibles much less to acci-
 “ dents from fire than the method of Dr.
 “ Smyth, since all other heat may be dispensed
 “ with except that produced by the mixture of
 “ the ingredients. In adding to this operation
 “ a small quantity of the oxide of manganese
 “ we procure the oxygenated muriatic acid
 “ gas, which I have proved to be the most
 “ sure preservative, the antiloimic by excel-
 “ lence, and which further recommends itself
 “ by the facility of appropriating it to all cases.”*

On the theory of action of the mineral acid
 gases, Morveau concludes thus: “ When we
 “ have burnt the clothes and furniture of an
 “ infected person was it ever suspected that any
 “ portion of the virus remained in the ashes?
 “ Are we not forced to acknowledge that the
 “ whole poison is destroyed by the combus-

* P. 251, &c.

“ tion? But if the poison be a combustible,
 “ obedient like all the others, to the affinities
 “ of the oxygen of the atmosphere, upon what
 “ ground can it be imagined that it will resist
 “ the condensed oxygen of our anti-conta-
 “ gious power, which operates so rapidly such
 “ astonishing combustions, which is the power
 “ *the most burning* that we know in nature?
 “ We need only then apply here, the defini-
 “ tions before established, that is, to call things
 “ by their right names; to declare, without
 “ waiting for further vain trials, that the poi-
 “ son attacked by the superabundant oxygen of
 “ the muriatic acid, will be as entirely *burnt* as
 “ by fire raised from any other combustible, and
 “ will be accompanied with heat, let loose by the
 “ oxygen of the air, in departing from its gas-
 “ eous state. I shall use another language to
 “ those to whom this doctrine is not familiar.
 “ Is it not proved by unfortunate experience
 “ that a letter sent from one infected by the
 “ plague

“ plague can communicate this terrible disease?

“ Is it not on this account that we never re-

“ ceive a letter from suspected countries which

“ has not been dipped in vinegar? When it

“ has undergone this operation you touch it

“ without fear, because you believe that any

“ poison sticking to it has been destroyed. It

“ is not necessary then to seek further for

“ proofs of the possibility of this destruction.

“ Your customs, formed after the example of

“ those of all other men, are founded upon

“ this principle, and declare conviction.

“ Compare now, only in their sensible effects,

“ the agent which has given you this security,

“ and that powerful anti-loimic which is offer-

“ ed to you in the oxy-muriatic acid. On

“ the one hand is a liquor placed in the lowest

“ order of antiseptics, which produces a very

“ slightly stimulating effect on our senses,

“ which seasons rather than alters substances

“ of taste, which *adds its own colour* to colours

“ the

“ the least fixed without separating the ele-
 “ ments of them, and whose action always flow
 “ is only efficacious upon bodies soaked in it.
 “ On the other hand is a subtle fluid which
 “ once disengaged, darts on its own wings and
 “ suddenly occupies the whole space of the
 “ largest habitations; leaves nothing untouch-
 “ ed, and touches nothing that it does not
 “ appropriate; which radically destroys colours,
 “ tastes, and odours the most virulent, which
 “ spontaneously inflames oils, sulphur, and
 “ metals; which breaks asunder the texture of
 “ all organised matter, and of which no living
 “ soul can receive the slightest impression
 “ without some extraordinary sensation giving
 “ notice of its presence. Such is the great in-
 “ strument of disinfection with which modern
 “ chemistry has made us acquainted, and which
 “ it has taught us to employ without danger,
 “ and with the certainty of obtaining a salu-
 “ tary result. I do not believe that any doubt

“ can remain that pestilential poisons, whatsoever shape they may assume, and whatsoever they may exist, will be reduced to the state of inert matter by the energy of its action.”

I must entreat my reader to pardon me, for the introduction of one or two more particulars, before I conclude this long and painful investigation of the subjects forced upon my mind, by Dr. Smyth's Letter to Mr. Wilberforce.

With respect to Dr. Withering's use of nitric acid gas, I never knew that he did employ it, although I was in the habits of friendly intercourse with him from August 1794 till his death ; nor did I know that he had published any thing about it in Duncan's Annals of Medicine till the result of his trials was pointed out by Dr. Smyth.* Whatsoever were the fact, so far as nitric acid gas was concerned, still Dr. Withering must have been previously

* Letter, p. 26.

acquainted

acquainted with my brother's recommendation of muriatic acid gas, for my brother James's Treatise on Malignant Angina, and Dr. Withering's book on scarlatina, were in some sort controversial; and in the latter work it appears to me, that alkalies are preferred to acids. With Dr. Smyth's publications I have long been acquainted, but they did not seem to me to require controversy, for they did not provoke it. The statement was inconsistent, the nitrous acid was extolled, the principle of discovery was not explicitly urged, and though a sort of claim to originality was irregularly scattered and indistinctly hinted, yet I had "no wish to criticise any man's writings, " however liable they might be to censure, " unless when provoked by insult or impelled " by duty." When Dr. Smyth's petition was presented to Parliament, claiming reward for discovery of the principle, I *then* was impelled by duty to assert my father's claim, and even

then I did not criticise Dr. Smyth's writings. I plainly stated my father's case with as little reference as possible to Dr. Smyth. It is now only that I feel myself called upon to criticise his writings, when I am provoked by insult.

Let me assure Dr. Smyth that it was no idea of the reward annexed to discovery that first struck me, and that first impelled me to vindicate the claim of my father. It was Dr. Smyth's exclusive claim that struck me for the first time,* THEN, when his petition was presented to the House of Commons. My father was THEN dying, and therefore did I become "the claimant for my father, and champion of the family."† But it was not reward that impelled me in the first instance, except indeed the consciousness of having discharged my duty to my venerable old father, can be called such. I believe I have at least as clear a perception as Dr. Smyth, *quantum inter laudem*

* Letter, p. 27.

† Letter, p. 25.

et lucrum interficit. The pecuniary reward should despise, without the praise.

Falsus honor juvat, et mendax infamia terret,

Quem nisi mendosum ?

The maxim which Dr. Smyth has chosen to apply to me, and which is next in order to my “first Essay,” *

Quis enim virtutem amplectitur ipsam,

Præmia si tollas ?

* Referring to an article of the Morning Chronicle, March 12, 1802, and imputing it to me, Dr. Smyth quotes the following words: “It still remains to be proved whether the diffused vapour of muriatic be not equal in efficacy, and equally convenient in application with the nitrous acid vapour for correcting vitiated air in ships, prisons, and hospitals.” In a tone of contemptuous exultation the Doctor subjoins, “and thus in 1782 (1802 it should be) it still remained to be proved what in his pamphlet he affirms to have been proved fifty years before.” Upon this sarcasm I must remind the Doctor, *non sat commode divisa sunt hæc temporibus.* TER. ADELPHI.

The absurdity lies not in my calculation but in Dr. Smyth’s miscalculation—not in my affirmation but in his mistatement—not in my logic but in his pleasantry. I never affirmed, nor even hinted, that the efficacy or the convenience of the muriatic acid gas, as compared with the efficacy and convenience of nitric acid

he himself has not disavowed practically. He acted upon it when he wrote his memorial to Lord North; he acted upon it when he presented his petition to the House of Commons. And in the interval between the application to Lord North and the publication of his book on Jail Fever, "his humanity did not revolt at the idea" of forbearing to publish the result of his experiments at Winchester, and in hospitals and private practice.

The undisguised and unceasing supercilious-
acid gas, had been established by my father fifty years before, or that they had occurred to him at any later periods of his life as subjects of comparison. I contended for his use and his knowledge only of the muriatic acid gas, long before the nitric appears to have been preferred or even used by Dr. Smyth. Whether the one be equal in convenience or efficacy with the other, was a doubt merely my own. I was not satisfied with the experiments which Dr. Smyth employed to solve it, and in order to obtain fuller satisfaction I made myself several experiments, and I collected from other men that information which is introduced into my pamphlet, and in which, my confidence is certainly not shaken by the passages which Dr. Smyth opposes to it.

ness

ness with which Dr. Smyth has been pleased to treat the character of my father compels me to protect his insulted memory, and the reader I trust will give me credit for intending not to lavish upon him, superfluous and ostentatious panegyric, but to vindicate him, by well-merited and extorted praise against contumely. My father had never, like Dr. Smyth, been honoured with any public commission from the authority of government. He never had access to patrons who were powerful in their influence and munificent in their favours. He* was destined to be, and he was content with being, a country physician. But his powers and his attainments are not to be measured by the accident of his situation. On the contrary his knowledge was at once comprehensive and

* I know not any physician who conformed more exactly to the rule laid down by Hippocrates in his Epidemics, lib. 1, 5, and thus luminously expressed by Celsus: "*Mederi oportere, et communia et propria intuentem.* In *Præfat.*

accurate—his application both to books and to professional duties was intense—his apprehension was quick—his memory was retentive—his penetration was keen—his judgment was correct—his elocution was ready, copious, and energetic—his practice was long and extensive—his employers were numerous and respectable—and in the catalogue of his private friends he had the honor to recount the names of men eminently and deservedly distinguished for elegant taste, for classical erudition, and for deep researches into all the various branches of philosophy, which are intimately or remotely connected with the study of medicine.

Nihil is vidit, neque in vita, nec gratia, nec rebus gestis, neque ingenio, quod despicere deberet Smythius.—CIC. PHILIP. ii.

I do not mean to depreciate the abilities of Dr. Smyth, or to deny the importance of his services, when he attended the Spanish prisoners at Winchester, and superintended or directed

rected the experiments that were made on board the Union hospital ship. I think him entitled to the splendid reward which he received from Parliament, by various and solid merits both of personal exertion and personal suffering; by his diligence in examining, and his judgment in applying the powers of nitric acid gas, and by his copious though managed communication, of his various experiments to the public. But I contend that for the discovery of the principle my father has a substantial and invincible claim to priority; and in regard to the agents, I maintain that the muriatic, *to say the least*, is equally useful in destroying contagion, and equally convenient in application with the nitric.

Upon questions of science when the members of a Parliamentary committee are requested to decide not on the merits of theories but the evidence of facts, I had been accustomed to look upon them both as honor-

able patrons, and as competent judges. I therefore wished to receive from Parliament some mark of favour, not for myself be it remembered, but for my father, to whose memory I should gladly have erected a monument with any sum which might have been granted for that purpose; or for some other branch of my family, upon whom Government in its wisdom might have been pleased to confer such, and only such patronage, as would have implied assent to the proofs, which I had adduced of the claims of my father. That wish I stated in my private correspondence with Sir William Pulteney, and I now abandon it with regret indeed, but without anger.

Whatsoever may have been the causes of my disappointment, and whether or no they may have been more within the ken and under the controul of Dr. Smyth than they were of my own, I forbear to inquire. But my filial piety and my sense of justice, will be satisfied
by

by the consciofnefs of my endeavours fo to
eftablifh the pretensions of my father, as fhall
fecure both refpect to his talents from his me-
dical brethren and from men of fcience, and
gratitude for his fervices, not only from his
contemporaries but from pofterity.

THE END.

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